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ABSTRACT

The proceedings include the following papers: "Why We Study Child Language"; "Children's Use of Information in Word Learning"; "An Examination of the Initial Mapping of Verb Meanings"; "Evidence for the VP Constituent from Child Korean"; "The Role of Stress, Position, and Intonation in the Representation and Identification of Early Words"; "Nonegocentric Uses of 'Big' and 'Little' by Preschool Children"; "Texts within Texts: A Developmental Study of Children's Play Narratives"; "Another Look at Children's Interpretation of Complements to 'Be Easy'"; "Precocious Passives (and Antipassives) in Quiche Mayan"; "Encounters with Japanese Verbs: Categorization into Transitive and Intransitive Classes"; "Language Learnability and Empirical Plausibility"; "The Acquisition of Reflexives and Pronouns by Icelandic Children"; "Linguistic Representation of Children's Wh-Questions"; "Word Learning Strategies in Two-Year-Old Children: Evidence for Category Hierarchies"; "Iconicity in Semantics: A Case of Suprasegmental Marking in the Acquisition of the English Plural"; and five presentations for a panel on future directions: "Confessions of a Wayward Chomskyan"; "What Is Functionalism?"; "A Principles-and-Parameters Approach"; "Theory and Explanation in Language Acquisition"; and "The Connectionist Approach to Language". (MSE)

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Editors' Note

This issue of *Papers & Reports on Child Language Development* contains the proceedings of the Twentieth Annual Child Language Research Forum, held at Stanford, April 8-10, 1988. We thank all the members of the CLRF committee whose hard work made the Forum possible; Charles A. Ferguson for giving the Keynote Address; Jane Grimshaw and Marlys Macken for organizing and leading Special Interest Groups; Dan I. Slobin for chairing and leading the Panel on Future Directions; and all the participants who contributed so much to making the meeting a success.

Next year's Forum, on **April 7-9, 1989**, will be coordinated by Sik Lee Cheung, Bonnie McElhinny, and Elizabeth Owen. The Keynote Speaker will be Lila R. Gleitman of the University of Pennsylvania. Abstracts (limit 250 words) will be due on January 10, 1989. Abstracts and enquiries should be addressed to Coordinators/CLRF-89, Department of Linguistics, Stanford University, Stanford, CA 94305.

August 1988

Eve V. Clark
Yo Matsumoto

Previous issues of PRCLD: Articles from previous issues of *Papers and Reports on Child Language Development* are generally available through the ERIC Document Reproduction Service, 3900 Wheeler Avenue, Alexandria, Virginia 22304, U.S.A. (Many articles that appeared in preliminary form in PRCLD have since been published in professional journals in the field.)

PRCLD 27 (1988)

Why we study child language

Charles A. Ferguson
Stanford University

It is a happy occasion when one can spend an hour explaining and justifying one's professional interests to a captive audience. I first became interested in the study of child language development as a graduate student in the 1940s when I took a course in the psychology of speech and also had the good luck to read Jakobson's Kindersprache, which had just appeared. But I wasn't able to devote any time to research or teaching on child language until twenty years later. From the 60s to the present I have been able from time to time to devote my attention to child language as one of my professional interests. In my remarks this evening I have no intention of trying to analyze how the events of my own life history might have led me to study language development or what personal goals or motivations might have been involved. The question I would like to address is the more general professional question of what legitimate intellectual issues justify the systematic study of child language, a topic at first glance far removed from serious academic disciplines in the arts and sciences.

There are, of course, many reasons for studying how children learn to understand the talk of people around them, to produce their own talk, and in many societies to read and write as well. I'd like to talk about four sets of reasons that have seemed important to me.

The first of these are, I suppose, biological reasons. We want to understand our own species and where it belongs among other living beings and in the universe as a whole. The most obvious place to start is in finding out how our body works, and this means observing the changing forms and functions of the body from conception to death, what could be called developmental biology. In recent years, to be sure, developmental biologists have become so justifiably fascinated by genetics and molecular biology that they hardly ever get past birth in their studies, let alone the maturation through childhood to adulthood. Yet in our species, as in many others, the development between birth and adulthood is not only as complex and mysterious as the first cell divisions and fetal development but is even more distinctive of the species in view of its neoteny and, of course, it is closer to the adult form and function and hence presumably easier for us to grasp, at least in a preliminary way.

One of the most salient characteristics of our species is the complex system of intraspecific communication that it exhibits. Other animal species, and even plants, have their own communication systems, but the humam system seems by orders of magnitude more complex, more pervasive, more central to the existence and flourishing of the species. Just as we have a digestive system, a circulatory system, a reproductive system, and so on, it makes biological sense to say we have a communicative system. The fact that most of the organs involved in the perception, internal processing and

production of language are also used for other functions does not exclude the complexly coordinated "system" nature of communication. Every normal human child, given a certain minimal exposure to language in use, acquires an incredible array of fantastically coordinated behaviors of language and related communicative activity. The obvious and almost completely unanswered questions are: How do we get from genes to adult language behavior? How did our species get from no language to language? The most obvious place to start to answer these questions, in spite of undoubted pitfalls and deadend byroads, is the study of child language.

Let us take an amateur ethologists's approach. Can we find some insightful analog to the development of the complex set of communicative behaviors? My favorite is one that Bickerton has often used, the nest building of various species of birds. At the appropriate point in the life cycle, birds of a given species engage in a set of behaviors of gathering materials, taking them to a selected place, and constructing a nest of a certain size, shape, and form of construction, and depending on the species, going through this routine only once, or annually, or several times a year. The point of the analogy is that the birds seem to know how to do this. Each species has a typical range of preferred materials, locations, sizes, shapes, and forms of construction, but of course the exact materials, etc. will be in part determined by what is available. It is certainly tempting to think of human language in these terms. Members of our species have in their genes (combined of course with the necessary interaction with the environment, including fellow members of the species), a fundamental plan for language-building. The exact details will, of course, depend on the particular materials available. If the materials available in the environment are the sounds and meanings of Chinese, the resultant language that is built will be Chinese-shaped; if the materials are the sounds and meanings of French, the resultant language will be French. But the basic plan of language-building will be the same. That is, there must be some set of genes the interaction and development of which specify this basic plan and its adaptability to local materials.

It takes only a few moments of consideration to find serious flaws in this analogy. Not simply that human language behavior is more complex than avian nest building, nor that it is proving extraordinarily difficult to discern the universal plan underlying both Chinese and French. These two differences -- the complexity of language and the difficulty of specifying Universal Grammar -- could be acknowledged and the insightfulness of the analogy still pursued. What for me destroys the usefulness of the analogy is the existence of multiple language building. A human child raised in a French-Chinese bilingual environment will build both nests/languages simultaneously. The genetic endowment of the child is such that he or she is able to discover that the input contains two sets of materials that are to be treated selectively and built in two different ways.

This capability of the human "faculty of language" cannot be overemphasized. Human beings can acquire different languages simultaneously or successively. A second or nth language may be acquired in childhood or

old age; from spoken, written, signed, or other input. A child raised in an environment with both English and American Sign Language will be bilingual in those. But child bilingualism is not the main point of this variability in language building. A little closer to the main point are the phenomena of child bidialectalism. Children raised in an environment where two dialects of the same language are spoken may acquire only one of the varieties, or both (i.e. become bidialectal), or may create a compromise variety different from either. And this whole picture must be suitably multiplied by numbers of possible dialects or languages. Humans are incorrigibly language building, even though in our dominantly monolingual American academic community we may lose sight of this.

The main point of variability in language building is actually slightly different from the bilingual/bidialectal issues. Human adult communicators vary their signals depending on the identity of the addressees; the time, place, and purposes of the communication; the content of the communication; and a host of other variables. Put more generally, language users show "register" variation, i.e. variation depending on occasions of use. In our own speech community, for example, we know that sports announcers' talk on the TV, mothers' talk to young children, teenagers' phone conversation, and classroom lectures are different kinds of English. People on different occasions may have different meanings to communicate, but even when they have essentially the same meaning to communicate they do so with different lexicon, phonology, syntax, forms of discourse, and, of course, means of non-verbal communication. Every normal adult human being has an extraordinary range of register variation, and some individuals have fantastic special competences in this regard. More important for our consideration, all children demonstrate register variation even before they produce their first recognizable words of the ambient language. Very early, children learn to communicate differently with familiar caregiver versus adult stranger, with mother angry versus mother playful, and so on. As Andersen (1984) and others have shown, four-year-olds can be very skillful at register variation. They not only have built different ways of talking to younger siblings than to peers, and so on, but they are able -- when suitable elicitation techniques are used -- to demonstrate how they think fathers talk to mothers, patients talk to nurses, and teachers talk to pupils, even though their own experience with these occasions of use has been quite limited.

Other animals show some differences in their signaling depending on the occasion -- watch a community of gibbons interacting -- so in principle this is not a surprising characteristic of human language. But the complexity and subtlety of this variation is a fundamental property of human language, perhaps more important than many of the other so-called "design features" that have been listed. It seems likely to me that this capacity to vary one's language registrally is fundamental to many of the other surprising communicative capabilities of humans. In a bilingual community where one language is largely ergative and the other essentially nominative-accusative in structure, children's ability to sort them out and acquire both is more deeply anchored in their capacity to adjust communication to context than it

is in some kind of grammar evaluating or parameter setting, important though such or similar processes may be from other perspectives.

If we ask ourselves what is likely to be found in that 1% or so of genes by which the human species is said to differ from chimpanzees that would make human language possible, my suspicion is that an expansion and reorchestration of communicative adaptability would be there. Human beings pay great attention to the cooccurrences of signals and contexts. The extreme situatedness of human communication is one of the big factors to explore. And the study of the growth and implementation of this capacity in the child's language would well repay the investment in time, effort, and research ingenuity that would be required.

I cannot leave the realm of biology without recalling an encounter I had some years ago in jointly teaching a course on Language and Mind with another linguist. It so happens that I was raised in a religious tradition and have myself a Christian commitment, but in that course I found that my curiosity was about how the human species came to have language and what aspects of human language were shared with other species, an evolutonary perspective. My colleague was raised in a secular tradition, and had, as far as I knew, no particular religious commitment, but his curiosity was about the shape of the universal language plan, not about any possible connection with other species, a clearly creationist perspective. The irony of the contrast was not lost on either of us, but we continued in our respective curiosities.

Well, so much for biology.

The second set of reasons for studying child language might be called "social". The human species like many otbhers, ranging from the so-called social insects to various mammals, is highly social, in the sense that there is extensive role specialization depending on one's place in social groupings, and many behaviors are explainable in terms of contributions to the community rather than for one's self and immediate family. In two ways, however, the human species seems qualitatively different from others in social behavior -- the existence of cultural differences and the pervasiveness of the continual construction and reconstruction of social reality by communicative interaction.

Every social group is characterized by a set of beliefs, values, material objects, and practices of all kinds that are learned and shared by members of the group and transmitted from one generation to the next. Each culture, that is, each particular structured aggregate of such features, is remarkably persistent over time even though continually changing in details and even in aspects of its overall configuration. Although some marginal instances of cultural differences between social groups can be found in other species, this business of having extensive behavioral differences between populations of the same species is one of the outstanding characteristics of human beings and hence something to be investigated and understood.

One of my favorite examples of the persistence of culture over time is the history of interaction between German missionaries in Australia and aboriginal groups they chose to preach their message to (cf. Fergson 1987). The best known story is that of the mission to the Aranda in central Australia begun in 1877.

The German missionaries took for granted not only the unique validity of their Christian doctrine, but also the universality of a set of valued practices, such as:

1. fixed times and places for eating, sleeping, worshiping, working;
2. personal cleanliness and elaborate clothing;
3. patrilineal family names and a simple kinship system with few constraints on marriage partners;
4. raising food crops, keeping domestic animals, and working for payment;
5. ownership of real and personal property, saving resources for future contingencies;
6. speaking a superposed standard German in addition to local dialect;
7. pervasive role of mother-tongue literacy: books, letters, records, lists, diaries;
8. pervasive role of numeracy: counting, measuring, comparing, doing arithmetic;
9. formal schooling for boys and girls, including studying foreign languages.

The Aranda took for granted traditional beliefs in an ancient dreamtime when their superhuman ancestors roamed the land, and also such cultural features as:

1. nomadic existence, moving in small groups from place to place in search of food, building temporary shelters or none at all;
2. no notion of personal cleanliness, and little or no clothing;
3. no family names but a very complex system of generational kinship classes that severely constrained possible marriages and prescribed patterns of social avoidance plus totemic clans that identified "ownership" of ceremonies tied to natural features of the land;
4. hunting and gathering, with fantastic abilities in tracking and hunting (a young boy might be able to identify the footprints of each of 200 persons meeting for a ceremony);
5. very limited real property, land ownership primarily in terms of the right to control appropriate ceremonies, no saving, eating food immediately when found but elaborate obligations for sharing food items with various relatives;
6. interaction across language groups, people picking up a different patrilect or language as needed;
7. no notion of literacy;
8. no notion of numeracy: Aranda has number words for one, two, three, and no higher; expressions for "more" and "less" rarely used; no notion of measurement; (yet able to quickly identify playing cards by non-numerical

classification, and playing children's game of star "counting" by matching configurations, without numbers);
9. formal teaching only with regard to details of kinship terminology and the traditional mythology and ceremonial lore in preparation for secret initiation rites of boys; only documented example of intellectual pursuits the discussion of the kinship system. ("...considerable enjoyment and satisfaction was derived from erudite discussions of the ramifications of the system, to which a great deal of time was devoted. One cannot but conclude that the pleasure was an intellectual one and that the activity was analogous to those indulged in by academically minded people the world over." Seagrim & Lendon 1980:34.)

Even from this brief summary the extreme cultural clash is quite evident. By the 1950s and 60s, however, the two groups had made considerable adjustment to each other and to mainstream Australia. The German missionaries acquired English as a second language, often speaking more bookishly than the English-speaking Australians, and subsequent generations moved from bilingualism to English dominance and finally English monolingualism. The Aranda began to acquire English and by the 1960s most of them knew some English, many of them speaking a somewhat pidgin-sounding non-standard variety, but some of them fluent and even English dominant. Most of the Aranda had family names and Christian given names. The Aranda had become staunchly Christian and there were several ordained Aboriginal pastors. Hundreds of Aranda were settled around the mission center of Hermannsburg. Aranda hymn singing was enthusastic, Aranda children were attending school, Aranda adults had learned to wear clothes and buy and sell.

But in the 1970s an astounding series of events took place. As the mission administrators moved first to encourage the formation of an Aboriginal council and Aranda policy making and finally to turn the title of the mission reserve to the Aranda, they were surprised to find that the Aranda did not recognize the authority of the council they had themselves elected, and instead of accepting overall Aranda ownership of the land, small groups began to "secede" as it were and move out to traditional ceremonial sites. It turned out that in spite of the apparently thoroughgoing changes in Aranda behavior, the old kin names and totemic identities had been maintained, knowledge of traditional places "owned" was largely intact, and many boys had been initiated secretly to avoid offending missionary and church leaders. The majority of the Aranda settled in Hermannsburg reverted to a life style much closer to the traditional. The new social groups were all eager for certain kinds of schooling and arranged to have one teacher assigned to each group, giving instruction outdoors in a way that was hard for Australian government authorities to fit into established patterns of education.

We might add here that it remains true that many Aranda children are good trackers with impressive observational skills but poor at literacy and numeracy even after years of schooling. Children with the least European type of experience were often Piagetian non-conservers at age 9 and 10; in

fact some adult Aranda do not believe that water poured from one container to another of different shape is necessarily the same amount.

The mystery is evident. How do social groups succeed in conventionalizing their particular cultural configuration? How do they manage to transmit the pattern to succeeding generations? How do they manage to change the pattern in response to external events and to internal forces for change in their own group? These are all the same question: How does a culture become conventionalized, i.e. shared by a group?

Language is at the same time a prime example of socially conventionalized behavior and one of the principal means of learning, sharing, and transmitting all of culture. Consequently one of the most obvious places to start in the attempt to fathom the mystery of conventionalization is with language behavior. For example, how do the members of a speech community come to share the thousands and thousands of details that constitute their mental lexicons? A neurologist has been quoted as saying "I must confess that I have always been more impressed with the capacity of the human brain to discriminate, characterize, and store in memory the thirty thousand plus arbitrary words in active use than with the complexity claimed to be involved in learning a few dozen syntactic algorithmic rules" (Marin 1982 as quoted in Aitchison 1987:202). The neurologist may well be impressed by the individual's astonishing achievement here, but I am even more impressed by the fact that members of the community share to an incredible degree this mass of detail. And, of course, even if we did put the lexicon at the center of language there would still remain the countless details of syntax, phonology, forms of discourse,and all the rest of language behavior that are shared. Some substantial part of this social competence may be in some sense universal and built in, but any speaker of English learning Aranda or any speaker of Aranda learning English or German will be forced to recognize an enormous mass of detail that distinguishes one language fom another, a mass of detail that is socially shared in the target speech community.

There are at least three places where the study of language conventionalization seems very promising: the study of language change in progress, the study of second language acquisition, and the study of language development in the child. In recent years, beginning with Labov's (1963) study of a sound change on Martha's Vineyard, we have seen more and more sophisticated study of language change in progress, and the workshop tomorrow afternoon will offer us a number of perspectives on this kind of research. The study of second language acquisition is just beginning to achieve the kind of sophistication needed for progress in understanding this kind of conventionalization, and recent shifts in grammatical theory and new approaches to discourse analysis are stimulating SLA research (cf. Flynn & O'Neil 1988 and Ferguson & Huebner, forthcoming). But of the three possibilities I find the study of first language acquisition the most promising. Of course, since at least in our society, and some other similar ones a crucial stage is when the child shifts from attending to the models of early caregivers to the models of child peers, we need careful

longitudinal research that combines more traditional psycholinguistic study with the sociolinguistic research methods of such linguists as the Milroys, Payne, Lodge, and Eckert.

Another comment here. Some outstanding developmentalists have conducted extensive cross-sultural studies of socialization, the most famous probably being the Whitings. But sadly, from my perspective, they paid no attention to language as such. Fortunately, a few child language researchers are attempting to repair this lack, and we have had Schiefflin, Ochs, and Heath reporting on their research at this Forum in other years.

In a fuller version of this talk I could deal further with the interactional construction of language conventionalization and change, but let me now just move on to a third motivation for child language study.

The third set of reasons for studying child language are cognitive. Each individual human being "knows" and "thinks" and "reasons". Certainly among the most basic questions we can ask about ourselves are: What does it mean to say someone knows something? How do people come to know things they previously didn't know? What is happening when somebody figures something out? We like to believe that knowing and thinking are states or activities highly characteristic of our species, and if so they are well worth systematic investigation.

Of course, we all acknowledge that other forms of life also "know" and "figure out" at some level. I am not thinking of a one-celled organism that recognizes a novel item as food and succeeds in ingesting it. That kind of knowledge and problem-solving is too far removed from the kinds of human knowledge and problem-solving activity that are so intriguing. A little closer is the kind of knowledge the household dog has about the spatial location of various familiar parts of the environment: the dog can either head for or avoid a particular object not present, in order to accomplish particular goals. As a member of a different species I find this example a familiar kind of knowledge. Presumably the dog can't talk to itself about the location of objects, but it has indeed come to know where they are.

I also recognize some dog behavior as problem solving of a familiar human type even though without all the attributes of the human activity. My own favorite demonstration was by a large, brown, standard-size poodle named Kittelbane who outwitted us many years ago. He liked to get out and roam around the neighborhood, causing a great deal of trouble, and we decided to build a fence around our back yard. With a considerable expenditure of money and time a six-foot high metal chain-link fence was built around three sides of the yard, the back of the house constituting the fourth side. I remember what a sense of satisfaction we had when the fence was completed and we opened the back door for the dog to run out into the yard. To our dismay he seemed to run immediately right through the fence and into the open neighborhood. Some careful investigation finally made it clear that there was about a six-inch space between a metal fence pole next to the house and the house wall, and the dog managed to squeeze himself through

that narrow opening and out into freedom. It was hard not to conclude that he had sized up the problem, had discovered a solution, and then was able to put it into immediate operation when the moment came. It may have been low-level reasoning but it was impressive, and it would be good to know just what went on in his central nervous system as he solved that problem.

Of course, human knowing and thinking are at a much higher level of complexity. Not only are the information to be known and the problems to be solved more complex, but our species has additional devices that may be used in these processes, such as the kind of inner speech that serves as an aid to thinking or a vehicle of thinking. To investigate how humans know and reason we could look at many different kinds of phenomena, and indeed cognitive scientists have done so. My own preference, as is natural from my educational and professional background, is to look at language. What does it mean to say that someone "knows" a language, i.e. can understand and speak a language (or for that matter read or write or sign a language)? How do people come to know a language they didn't previously know? How does people solve the problems of putting utterances together to say the things that they want to say, and how do they succeed in interpreting what someone else is saying?

One of the reasons for choosing language as the focus of cognitive research is that it is both highly complex and well described. Linguistic publications offer incredibly detailed accounts of certain aspects of language behavior, even though the authors may not regard description of behavior as their goal. This means that the experimentalist has a rich supply of well described bodies of knowledge to explore. A second reason for choosing language as the focus of cognitive research -- one that is reminiscent of Labov's 1964 criteria for selecting phonological variables for sociolinguistic research -- is the fact that most of the intricate patterning of language behavior is out of awareness, not easily accessible for verbalization by the language user. Naive speakers of English, for example, may be totally unaware of how they vary the length of vowels in relation to the voicing of a following consonant or the complex constraints they obey on forms of anaphora or the syntactic or discoursal conditions for their use of verb inversion. There is a further reason for the focus on language: the very terms of linguists' accounts -- the primes, rules, principles, representations, and the like that linguists put in their descriptions -- may be regarded as hypotheses about the actual processing of language by hearers and speakers. Investigators have, as it were, whole sets of ready-made hypotheses to be tested in whatever ways their ingenuity can devise. Of course, cognition-oriented research on language should not be seen merely as the testing of linguistic theory, but rather as an independent line of research that may indeed test linguistic theory or may stimulate new kinds of linguistic theorizing or may contribute to scientific understanding of our species that has little to do with linguistics.

But why <u>child</u> language? One deceptive attraction some of us have felt is the apparent simplicity of child language as opposed to adult language. Child language at any stage is presumed to have fewer words, fewer sounds,

fewer rules than the full language of the adult and hence might be analyzed more readily and once analyzed might give us valuable guidance for analyzing adult language. Alas this is not as valid as it has seemed. The route from zero to full competence is not a simple linear progression of successive approximations to an adult system. It was a great insight of Jakobson that child language at any stage is structured in the same senses that an adult language is structured although differing in detail, and this was an important step forward in the understanding of language development at the time, but it did not allow for the extensive individual differences in paths of development. These differences may come from differences in input, differences in cultural patterns of language socialization, or -- possibly of greatest interest to the cognitivist -- the varied choices of strategies and individual creativity on the part of the child.

No matter where one stands on the question of the relation between some kind of general cognitive ability and linguistic competence, the study of child language is a promising field. My own preference on this question, as you might expect, is for research that tries to discover relationships between specific non-linguistic cognitive skills and specific linguistic skills rather than global comparisons or claims of overall priority or autonomy. Fortunately I don't have to say much about cognitively-oriented research on child language, since -- as nearly as I can tell-- every paper on the program of this year's Research Forum is an example, I will limit myself to one small topic: the "acquisition of a rule".

In what is rightly referred to as a "classic paper" Berko [Gleason] in 1958 showd us that preschoolers could say <u>wugs</u> as the plural of a word <u>wug</u> that they had never heard before; that demonstration convinced a whole generation of psycholinguists that children acquired not just words and sentences but rules for constructing words and sentences. That article reflected a shift taking place from talking about "habits" to talking about "rules", a shift that has on the whole been valuable for increased understanding of language behavior in general and language development in particular. What I want to comment on tonight is my understanding of <u>wugs</u> 30 years later. Just four points,

1. Acquisition. If one can show that a child will extend a particular pattern to a novel item, this does not necessarily show that a child has acquired an adult rule. One must first show that the adults have such a rule and that the child has discovered the adult rule and puts it into operation in his/her own language production. Part of the problem is demonstrating the existence of a rule in the input languge. I have told elsewhere of my disillusionment in discovering that individual adults vary greatly in their formation of plurals of nouns ending in voiceless fricatives, and that the problems of finding the source of the children's patterns are severe (Ferguson 1979). The patterns of generalization to nonsense material may be quite different in adult and child, with corresponding problems in deciding on rule acquisition.

2. Habits. Psycholinguists, after some chastening research experiences,

have concluded that not all generalizations to new material represent rules in operation. In a recent article in Cognition Lachter & Bever assert, "The emergence of overgeneralization is not unambiguous evidence for the acquisition of a rule. Rather it may reflect the emergence of a statistically supported pattern of behavior" (239), and in the next sentence they unabashedly refer to such patterns as "habits".

3. Universals. Sometimes a pattern applied to new material has no source in the target language, but represents a universl tendency in language production. For example, when a child acquiring a language without final consonants attempts to reproduce final voiced consonants in nonsense stimuli, the child may devoice the final consonants even though he has a voicing contrast in initial consonants. Final consonant devoicing is a widespread phenomenon in the world's languages and in child language development.

4. Creativity. The child may create a rule which has some basis in the adult model but represents a wrong analysis. Indeed, some of the interesting details in the original wugs article are the incidental references to unique creations by particular children, noted at the time as amusing exceptions, but neglected in the subsequent research literature. They were eloquent testimony to the active, playful, system-building propensities of young learners, cognitive behavior more impressive in its own way than the acquiring of adult rules.

The moral of these points, if there is one, is that every advance in theory making or research methodology is likely to obscure some of the valid points of earlier theories or methods as well as providing clues to better ones, and my preference is always to look both backwards and forwards this way rather than to argue for the supreme correctness of a current method or theory.

Well, let me now turn briefly to the fourth kind of reason for studying child languge. I have tired your patience enough with the first three, and I'll spend only a minute or two on the last one. What I have in mind are interventionist reasons. Some people study child language because they are interested in education, remediation, or therapy. I hold the opinion that this kind of motivation is just as important as the other three, and I wish that more people interested in intervention would get involved in the study of normal language development. Some years ago when I first looked into textbooks on speech and language disorders I was surprised to find that very little space was given to normal development: more seemed to be known about deviations from normal than about the normal development they were presumably deviations from. Doubtless much can be learned from abnormal or atypical behavior that will help us to understand the more usual behavior, but the reverse is just as true, and it is good to see that educators and therapists are increasingly making use of biologically, anthropologically, and linguistically oriented research on normal development, in some cases contributing to it themselves. I have always been somewhat disappointed that we haven't given greater encouragement to the presentation of really

excellent intervention-oriented papers at our annual Research Forum, but I realize that there are other, larger forums for such papers.

In the preceding sections I have reviewed some biological, social, ognitive, and interventionist reasons for studying child language. I am not presenting these assorted reasons as a new approach to the study of child language. On the contrary, I am trying to recapture some of the awe-struck, interdisciplinary enthusiasm of the first modern students of child language before our various specializations narrowed our vision. I think, for example, of William Preyer, whose 1882 book Die Seele des Kindes (English title The mind of the child) exemplifies almost exactly the kinds of reasons I have discussed. I do not share Preyer's political views, and I feel his understanding of cultural differences was too biological for my taste, but he called for (and contributed) repeated observations of the same child, observations of different children, comparisons of animal and human behavior, anthropological comparisons (with children "in uncivilized nations") and comparison of normal and abnormal development. And all this was in an evolutionary framework and with a strong physiological bias. Unfortunately he accepted Haeckel's strict recapitulationism as a principle, but fortunately he ignored it when his own empirical observations suggested otherwise.

Let me close with three one-sentence quotations from Preyer that will show illustrate strengths and weaknesses (from Die Seele des Kindes as quoted in Eckardt et al. 1985:183):

"There is no generally valid and temporal sequence of sounds in the language of children, because each language possesses another sequence of sounds based on their frequency (in the respective language)."

Here Preyer pays no attention to the universal tendencies in human language phonology, pays some but not enough attention to the distinctive phonological organization of every language, and somewhat too much attention to the role of frequency. Jakobson and many child phonologists are the mirror image. They pay too much attention to phonological universals, some but not enough to the phonological organization of each language, and no attention at all to the role of frequency. We are now in a much better position than either Preyer or Jakobson to summarize the phenomena of phonological development (cf. Vihman 1988).

"Heredity must be without influence because each healthy child, who hears a language from birth, which was unknown to his ancestors, will still learn to speak this language perfectly."

Here Preyer gets it only half right. Very much like American structuralists of a generation or two ago, he realizes that there is no genetic disposition for particular languages, but he misses the possibility of a common nest-building plan for phonology. Finally,

"What is hereditary is the great plasticity of the entire speech

apparatus."

Here Preyer says just what I want him to say. He is one with those recent researchers who focus on human plasticity (cf. the stimulating book on human plasticity Lerner 1984, which, however, fails to deal with language!). It is by careful investigation of the exact nature of the playful, structure-building plasticity in child language development that we can begin to answer some of the biological, social, cognitive, and interventionist questions that are posed for us.

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Children's Use of Information in Word Learning

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Children are constantly bombarded with information about new word meanings. Whenever they hear a novel word, the context supplies information about its meaning--more, probably, than they can ever keep track of. How do they cope? One strategy may be to make the most of whatever seems to make sense, given their prior knowledge and beliefs, while ignoring, or quickly forgetting, the rest. Obviously, if children process information in this way, they run the risk of wasting pertinent information. But they also stand a chance of using pertinent information very efficiently. This work examines whether or not children children's knowledge and beliefs about word meanings may affect their use of information in word learning. Central to the discussion are two general ideas that children seem to have about word meanings. First, some possible meanings look more plausible than others. Second, different words mean different things.

Preferences. Children seem to have preferences that are specific to word meanings. For example, they often interpret a new word as a label for an object category or a shape rather than as a label for a color, a substance (Au, 1985; Baldwin, 1986; Clark, 1973; Dockrell, 1981; Dockrell & Campbell, 1986; Macnamara, 1982; Soja, Carey, & Spelke, 1985; Taylor & Gelman, 1988), an object part (Markman and Wachtel, in press), or a thematic relation (Markman & Hutchinson, 1984). This preference persists even for adults (Au, 1985), and it does not appear in comparable situations where no novel term is introduced (Baldwin, 1986; Markman & Hutchinson, 1984). Both children and adults also have a material-over-color preference that seems to be specific to word meanings (Au & Markman, 1987).

The Principle of Contrast. Most, if not all, linguists hold that different words mean different things (e.g., Bolinger, 1977; Palmer, 1981). To capture this intuition, Clark (1983, 1987) proposes the Principle of Contrast, which states that every two forms contrast in meaning. There is some evidence that children do honor this principle in their word uses. Consider overextension. For example, young children sometimes apply the word *dog* not only to dogs, but also to other four-legged mammals such as cats, sheep, horses, and cows (Clark, 1973). When they acquire *horse*, they tend to stop overextending *dog* to horses, although they may still overextend it otherwise (Barrett, 1978; Leopold, 1949). This is just what young children should do if they think that a new word (e.g., *horse*) should contrast in meaning with the words that they already know (e.g., *dog*). See Clark, 1983, 1987, for more thorough reviews of the evidence.

Use of Information in Word Learning. It seems plausible, then, children think that some hypotheses are better than others and that different words mean different things. How may these ideas affect the way children use information in word learning? This discussion will focus on one kind of information that children often encounter, namely, linguistic contrast.

If children know how a new word is related in meaning to a familiar word, they may be able to narrow down the set of possible meanings tremendously. One way children may map a new word onto an appropriate semantic domain is to hear it contrasted with a familiar word from the same domain. For instance, if they know that *red* is a color word, and they hear a new word such as *beige* contrasted with *red*, they may infer that *beige* is also a color word. Children often hear adults contrast words explicitly when the adults correct the children's errors (cf. Brown & Hanlon, 1970). For example, when a child calls a muffin a cookie, the parent may say, "No, that's not a cookie. That's a muffin." In these cases, the contrasting words virtually always belong to the same semantic domain.

Au and Markman (1987) examined how children use this kind of linguistic contrast in

word learning. In that study, some children simply heard a novel word applied to a square swatch. (E.g., "Can you bring me the rattan [or mauve] square? See--this is rattan [or mauve]."). These children seemed to favor material over color in their hypotheses about the new word meaning. Other children received additional contrastive linguistic information. (E.g, "This is not wood, and this is not cloth. This is rattan." Or "This is not red, and this is not green. This is mauve.") When the information confirmed the preferred hypothesis--that the novel word referred to the material of the square--children were more likely to interpret their new word as a material name than the children who did not hear the linguistic contrast. However, when the information did not confirm their preferred hypothesis, children acted as if they did not hear it. That is, children who heard the novel word contrasted with two familiar color names responded much like those children who were not given any linguistic contrast.

An important question remains open is why children can use linguistic contrast effectively only in some situations. For instance, it is possible that in general children can take advantage of pertinent information only if it supports their favored hypothesis, such as material. But it is also possible that children can make use of pertinent information in most cases, and they fail to do so only when it supports a hypothesis that they believe to be wrong. That is, they failed to use linguistic contrast to learn a color name because for some reason they had a bias against color. One possible reason for such a bias is that categories named by color words do not have sharp boundaries. As a result, children usually have a color word that can be readily stretched for referring to a color they do not yet have a name for. So if children believe that words should contrast in meaning, they may think that a novel term, such as *mauve*, cannot refer to the color of the object to which the term is applied because they believe that a familiar term, such as *purple*, refers to that color. In other words, a familiar color name may stand in the way when children have the opportunity to learn a new one. In short, perhaps children can usually benefit from linguistic contrast--or other kinds of pertinent information--in word learning. However, they may fail to do so if the information supports a hypothesis that they deem wrong on the basis of their prior knowledge and beliefs about word meanings.

The present study examined how children's ideas about word meanings may affect their use of contrastive linguistic information in three domains: color, material, and shape. It focused on (1) children's preferences for certain hypotheses about word meanings, and (2) their belief that different words mean different things. A novel color, material, or shape name was introduced to each child. Some children simply heard their novel word applied to an object. Other children got additional contrastive linguistic information pertinent to the new word meaning.

Recall that Markman and I found that children preferred material to color in their hypotheses about a new word meaning. And previous studies of word learning suggest that children favor shape or object category over material (Soja et al., 1985; Taylor & Gelman, 1988). Therefore, children in the present study were predicted to favor shape over material over color. If so, it would be possible to see if children still used linguistic contrast about material even when material was not their preferred hypothesis, or if it was no longer helpful--like linguistic contrast about color in Au and Markman's study. Also, as discussed earlier, it seems that children may fail to use pertinent contrastive linguistic information to learn a novel color name because a color name they already know preempts a color interpretation for the novel word. In this study, it was possible to look again if such preemption does occur not only in the domain of color, but also in the domains of material and shape.

Method

Subjects

Seventy-two children from six preschools in northern California participated in this study. There were 29 girls and 43 boys. They ranged in age from 3;1 to 5;0 (mean age 4;2).

Stimulus Materials

The objects used for teaching children new words were swatches of different colors, materials, and shapes. Three kinds of material and three shapes were used with their appropriate names (*acrylic, plush, rattan; crescent, elliptical, trapezoid*) in the introducing event. Altogether there were nine stimulus swatches, including all possible combinations of these materials and shapes, each in a different color. The nine color words included *annato, celadon, chartreuse, fiesta, flaxen, infantry, leghorn, mauve, ocher*.

Procedure

There were four conditions: Label Only, Color Name Contrast, Material Name Contrast, Shape Name Contrast. The children were randomly assigned to the four conditions, with 18 per condition, approximately balanced for age and sex. The mean ages for the four conditions were 4;1, 4;2, 4;2, and 4;2, respectively.

Children were asked individually to come play a game for about ten minutes in a quiet corner of their school. Each of the nine stimulus figures was used for introducing one new word to two children per condition (one 3-year-old and one 4-year-old). The mauve rattan elliptical swatch can illustrate the procedure.

In the *Label Only Condition*, as a child approached the game table, I would point at a swatch a few feet away and ask, "Can you bring me the mauve [or rattan or elliptical] thing?" When the child handed me the swatch, I said, "See, it's mauve [or rattan or elliptical]." Six children heard a novel color name; six, a novel material name; and six, a novel shape name. These three kinds of names were randomly assigned to children, approximately balanced for age and sex.

In the other three conditions, children heard a novel word contrasted with two familiar words from the same semantic domain. In the *Color Name Contrast Condition*, a child might hear, "Can you bring me the mauve thing?" and then, "See, it's not yellow, and it's not green. It's mauve." In the *Material Name Contrast Condition*, a child might hear, "Can you bring me the rattan thing?" and then, "See, It's not paper, and it's not cloth. It's rattan." In the *Shape Name Contrast Condition*, a child might hear, "Can you bring me the elliptical thing?" and then, "See, it's not square, and it's not triangular. It's elliptical."

Testing Procedure. The testing session began about one minute after a child had heard a new word. Five tests were designed to find out what the children thought their new word meant.

(1) *Sorting Task*. In this task, the child saw four sets of four swatches. Each set included a "target swatch," namely, the swatch used in the introducing event (e.g., the mauve rattan elliptical swatch). Each set also included three other geometric figures: a color-associate (e.g., a *mauve* paper square), a material-associate (e.g., a green *rattan* square), and a shape-associate (e.g., a green paper *ellipse*). For each set of four swatches, the child would hear, "Is there a mauve [or rattan or elliptical] one here?" depending on which new word was introduced to the child earlier. If the child chose one, the child would then be asked, "Is there another mauve [or rattan or elliptical] one here?" Thus it was possible to see if the child chose the swatches on the basis of color, material, shape, or some other criterion.

(2) *Hyponym Task*. The child saw a blue paper square and heard, "It's not mauve [or rattan or elliptical] because it's...." The rationale for this test was that if the child thought *mauve* (or *rattan* or *elliptical*) was a color word, the child might say, "because it's blue." If the child thought it was a material word, the child might say, "...because it's paper." If the child thought it was a shape word, the child might say, "...because it's square."

(3) *Color Identification Task*. The child saw ten color chips including the non-focal colors chosen for the stimulus squares. The child was asked, "Is there a mauve [or rattan or elliptical] one here?" If the child chose a chip, I would ask, "Is there another mauve [or rattan or

elliptical] one here?"

(4) *Material Identification Task*. This task was identical to the Color Identification Task except that, instead of ten color chips, the child saw ten rectangular swatches of ten different materials including acrylic, plush, rattan, and sponge.

(5) *Shape Identification Task*. This task was identical to the previous two tasks except that the child saw ten paper swatches in various shapes including elliptical, trapezoid, pentagonal, and round.

Assessment of Availability of a Familiar Word

Another group of 3- and 4-year-olds were asked to name the colors, materials, and shapes of the nine swatches used for introducing novel words in this study. The questions used in these naming tasks were: "What color is this?" "What is this stuff?" and "What shape is this?" The naming responses could then be used for estimating to what extent the other children in this study--those who heard a novel word applied to one of these swatches--believed they knew the names for the color, material, and shape of their swatch. Naming responses were collected from 14 children. These children came from one of the six preschools that participated in the word-learning portion of this study. Altogether there were six girls and eight boys. They ranged in age from 3;2 to 4;11 (mean age 4;3). The order of these naming tasks was randomized and counterbalanced across children.

Results and Discussion

Three main findings are of interest. The first concerns whether children prefer some hypotheses about the semantic domain of a new word, such as shape, over others, such as color. The second has to do with children's ability to use linguistic contrast to induce the semantic domain of a new word. The third concerns children's beliefs about the adequacy of their vocabulary.

Preferences. This study reveals that children have a strong preference for shape in hypotheses about word meanings (see Table 1). This is consistent with findings in previous studies (Au, 1985; Baldwin, 1986; Clark, 1973; Dockrell, 1981; Dockrell & Campbell, 1986; Macnamara, 1982; Markman & Hutchinson, 1984; Soja et al., 1985; Taylor & Gelman, 1988).

TABLE 1: Mean Percentage of Responses Suggesting Various Interpretations

INTERPRETATION	CONDITION				OVERALL
	Label Only	Color Name Contrast	Material Name Contrast	Shape Name Contrast	
Shape Name	52	33	42	67	49
Material Name	27	27	50	22	31
Color Name	21	39	18	22	25

Children in the Label Only Condition gave more responses that suggested a shape name interpretation (52%) than a color name interpretation (21%), matched $t(17) = 3.34$, $p<.05$, two-tailed. There was also a trend toward giving more responses suggesting a shape name interpretation than a material name interpretation (27%), matched $t(17) = 2.03$, $p<.06$, two-tailed. The way these percentages were determined can be illustrated using the shape name interpretation as an example. In the Sorting Task, to be counted as having this interpretation, children had to choose members in the four sets of swatches on the basis of shape. In the Hyponym Task, they had to respond, "This is not X because it's square" (where "X" represents their new word). In the Color and Material Identification Tasks, they had to deny that any of the

color chips or swatches could be named by the new word. In the Shape Identification Task, they had to choose only the shape identical to the shape of the stimulus swatch originally referred to by the new word in the introducing event. If children refused to choose any shape or chose more than one shape haphazardly, they were not credited as selecting on the basis of shape.

Unlike what Au and Markman found earlier, this study revealed no reliable preference favoring material over color. In this study, children who heard a novel term applied to an object did not give reliably more responses that suggested a material name interpretation (27%) than a color name interpretation (21%), matched $t(17) = .86$, $p>.3$, two-tailed. Perhaps children's preference for shape was so strong that it pulled children away from considering material as a hypothesis for the new word meaning. Note that Au and Markman probably preempted a shape name interpretation by introducing the novel word with a square swatch and calling it "an X square" (where "X" represents the novel term).

Use of Information. Au and Markman also found that children favored material over color in hypotheses about word meanings, and that they could benefit from linguistic contrast to learn a material name but not a color name. As discussed earlier, this pattern of results could occur if (1) children generally can benefit from pertinent information only when it supports a favored hypothesis, or (2) children generally can benefit from pertinent information except when it supports a hypothesis in strong disfavor. The findings of this study went against the first possibility. Specifically, children showed a strong preference for shape over material and color in their hypotheses about word meaning. But they benefited little from linguistic contrast such as "It's not square, and it's not triangular. It's elliptical," in learning a new shape name. On the other hand, they did benefit from linguistic contrast such as "It's not paper, and it's not cloth. It's rattan," in learning a new material name. These findings were revealed by three analyses.

The first analysis computed, for each child, the percentage of responses that suggested a shape name interpretation, with the five tests combined and equal weight given to each test. These data were first submitted to a 4 X 2 X 2 (Condition X Age X Sex) ANOVA. This analysis revealed a reliable Condition effect ($F(3,56) = 3.26$, $p<.05$) and no age or sex differences. But this Condition effect did not generalize across stimuli, as shown by a 4 X 9 (Condition X Stimulus) ANOVA, $F(3,24) = 2.6$, $p>.05$. Children who heard their new word contrasted with two familiar shape names gave somewhat more responses that suggested a shape name interpretation (67% in the Shape Name Contrast Condition) than those who heard their new word applied to an object but heard no other information about its meaning (52% in the Label Only Condition). However, the difference was not reliable, $t(56) = 1.39$, $p>.1$, two-tailed. These results show that linguistic contrast such as "It's not square, and it's not triangular. It's crescent," did not reliably help young children learn a shape name even though it confirmed their preferred hypothesis.

The second analysis revealed that linguistic contrast such as "It's not paper, and it's not cloth. It's rattan," helped children overcome their shape-over-material preference to induce a material name meaning. This directly paralleled the shape name interpretation analysis. It revealed a reliable Condition effect that generalized both across children and stimuli (across subjects: $F(3,56) = 3.96$, $p<.02$; across stumuli: $F(3,24) = 4.38$, $p<.03$). Children who heard their new word contrasted with two familiar material names gave more responses that suggested a material name interpretation (50% in the Material Name Contrast Condition) than those who heard their new word applied to an object and heard no other information about its meaning (27% in the Label Only Condition). This difference was reliable both across subjects ($t(56) = 2.52$, $p<.02$, two-tailed) and across stimuli ($t(24) = 2.75$, $p<.02$, two-tailed). In short, children took advantage of linguistic contrast with material names even though it did not confirm their preferred hypothesis.

The third analysis revealed that children who heard their novel word contrasted with two familiar color names gave more responses that suggested a color name interpretation (39% in the Color Name Contrast Condition) than those who only heard the novel word applied to an object but got no contrastive information (21% in the Label Only Condition), $t(56) = 2.43$, $p<.02$, two-tailed. But this result did not generalize across stimuli, post-hoc matched $t(8) = 1.67$, $p>.1$, two-tailed.

In sum, while children seemed to prefer shape in hypotheses about word meanings, they did not benefit much from information that supported this preferred hypothesis. And while material did not seem to be particularly favored as a hypothesis in this study, children took advantage of information supporting this hypothesis. It does not seem, then, that children are always better at using pertinent information consistent with their favored hypotheses than at using information inconsistent with them. From here on, the analyses will examine the second possible explanation for Au and Markman's findings, namely, that children often fail to benefit from pertinent information in word learning when it supports a hypothesis in strong disfavor.

Beliefs about the Adequacy of Their Vocabulary and Use of Information. If children believe that different words mean different things, they should resist learning a new word that overlaps very much in meaning with a word they already know. Because of such resistance to apparent synonyms, children may sometimes fail to benefit from pertinent information about new word meanings. In order to examine this possibility, 14 3- and 4-year-olds were asked to name the colors, materials, and shapes of nine stimulus swatches. Their ability to come up with a name and their response latencies were used to estimate to what extent 3- and 4-year-olds felt that they already had names for these colors, materials, and shapes. When asked to name the rattan, plush, and acrylic materials, these 14 children often said they did not know what the material was. They did so for 41% of the trials, compared to 8% of the trials with the eight nonfocal colors (matched $t(13) = 4.96$, $p<.0001$, two-tailed). It also took children longer to offer answers during the material naming trials. For acrylic, plush, and rattan, children took on the average about 6.7 seconds before they responded, compared to about 2.5 seconds for the eight nonfocal colors (matched $t(13) = 4.81$, $p<.0001$, two-tailed). Note that children also benefited reliably from pertinent linguistic contrast for learning the names for these three kinds of material. They were less consistent in using pertinent linguistic contrast to learn the names for the colors.

The naming data for the shape items were more varied. Children seemed to find the trapezium hardest to name, followed by the ellipse. They named the crescent shape--they usually called it "moon"--extremely readily. Some item analyses were performed in order to understand these data better. It would be desirable to do comparable analyses on the color and material naming data. But unfortunately, because only two children heard each novel color name contrasted with familiar color names, the sample size was too small for making inferences about individual color items. So from here on, the analyses will focus on how much children benefited from pertinent linguistic contrast in learning the names for the material and shape items.

The analyses first computed the benefit of pertinent contrastive linguistic information, based on children's responses in the word learning task. For the material items, this meant the difference between the Label Only and Material Name Contrast Conditions in percentage of responses suggesting a material name interpretation. For the shape items, this meant the difference between the Label Only and Shape Name Contrast Conditions in percentage of responses suggesting a shape name interpretation. There were six children per item per condition. The variance in the data differed considerably from item to item, by as much as a factor of 7.4. So these analyses used the *t*-statistic of the increase in percentage of correct responses to estimate the benefit of linguistic contrast. Each *t*-statistic showed how much pertinent linguistic contrast encouraged an appropriate interpretation for each novel word, with

the variance in the data for each item adjusted to the same standard.

TABLE 2: Responses in the Naming Task and the Word Learning Task

Item	Naming Task		Word Learning Task		
	%"Don't Know."	Latency (seconds)	Benefit of Pertinent Linguistic Contrast		
			%Increase in Correct Responses	$t(10)$	p-level
rattan	29	6.8	27	1.39	.20
plush	36	6.3	30	1.50	.17
acrylic	57	7.2	16	2.08	.065
crescent	7	2.6	10	.46	.66
elliptical	36	5.2	20	.56	.59
trapezoid	71	6.1	43	2.21	.052

The benefit of pertinent linguistic contrast, as estimated by the t-statistics, was reliably related to how often another group of children failed to name the items (Pearson $r(4)=.86$, $p<.05$, two-tailed). The benefit was also marginally related to the response latency data (Pearson $r(4)=.77$, $p<.1$, two-tailed). These findings suggest that young children are better at using pertinent information to learn a new label for something if they do not already have a ready label for it than if they do. In the latter case, a familiar word meaning may stand in the way when children try to learn the new word meaning because children tend to resist synonyms.

Conclusions

In this study, some of the novel words introduced to the children seemed to pick out concepts that young children have ready labels for. For example, 3- and 4-year-olds seemed quite happy to name the colors of the stimulus swatches with color names already in their repertoire such as *purple* (for mauve), *green* (for chartreuse), *white* (for flaxen), and so on. They also seemed quite willing to call the crescent shape "moon." It is perhaps not mere coincidence that young children at this age also frequently failed to take advantage of pertinent contrastive linguistic information in learning novel names for these colors and shapes. In fact, this study suggests that children's success in using pertinent information to learn a new word meaning may be affected by their knowledge of other word meanings. Quite probably, children are most successful when they do not already know a word that has roughly the same meaning as that implied by the information.

To conclude, children may deal with the barrage of information in word learning by making the most of whatever seems to make sense, given their prior knowledge and beliefs, while ignoring, or quickly forgetting, the rest. They seem to find some hypotheses more plausible than others and rely on such preferences to pick out their initial favored hypotheses. Their idea that different words mean different things also seems to affect how they make use of pertinent information. When they hear a novel term, they tend to look for an as-yet-unnamed category as a candidate for the new word meaning. No doubt, children run the risk of wasting pertinent information if they only take advantage of information that makes sense to them according to their prior knowledge and beliefs. On the other hand, by being selective in taking in the information available to them, they may avoid being overwhelmed by information. Perhaps no less importantly, they also stand a chance of using pertinent information very efficiently.

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PRCLD 27 (1988)

AN EXAMINATION OF THE INITIAL MAPPING OF VERB MEANINGS

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It has become increasingly clear that children's learning of word meanings is best characterized as a rapid process that is directed at least in part by children's rough hypotheses about what novel words mean. This process, which has been called "fast-mapping", appears to be an apt description of how children learn the meanings of certain kinds of words, such as nouns and some adjectives, such as shape and color terms (Carey & Bartlett, 1978; Heibeck & Markman, 1987). Much less, however, is known about how children learn new verb meanings. Understanding what children assume a new verb means is important because verbs differ substantially from nouns and adjectives in their semantic organization, (e.g. Hutenlocher & Lui, 1979), and also because verbs and their argument structure are given roles of central importance in current theories of language acquisition (e.g. Bresnan, 1982; Pinker, 1984). This paper is addressed at precisely this issue: What are children's hypotheses about what a novel verb means upon their first exposure or exposures to that verb?

The theoretical framework that is used in this paper is one that claims that the mental representation of verb meanings, which I will be calling verb concepts, are best thought of as a type of schema such as a script (e.g. Schank & Abelson, 1976) or frame (Minsky, 1981), which belong to a class of representations that can be called "slot-filler" models (e.g. Nelson, 1986). The important feature of slot-filler models is that the representational structure has a number of placeholders, or "slots", that can be filled by a number of values, one or more of which may be more likely to occur or weighted most strongly in the representation. When studying children's assumptions about what novel verbs mean, we are interested in the default values for these slots; that is, the properties of these verb concepts before any information about the meaning of a given word is learned. These default values, according to a slot-filler model, serve to guide the verb learning process.

What information about verb meanings is contained in these slots? The two most commonly cited slots are action slots and result slots. Action slots contain information about the physical action performed by an agent in an event. Result slots contain information about the result of an event or the presumed goal of an action in the event. This distinction has been made commonly because some verbs explicitly label an action (such as "jump", "pound", "kick") without any reference to the result of that action, and other verbs explicitly label a result (such as "break", "melt", "clean") without specifying the action that produced the result. Most studies of the development of the verb lexicon and children's spontaneous use of verbs to label events have shown that action verbs tend to be used earlier and more frequently than result verbs by young children, with result verb usage increasing with age (e.g Behrend, 1987a; Bloom, Lightbown, & Hood, 1975).

A third slot of interest is an instrument slot. While an instrument slot is not essential to many verb meanings, it is of potential importance because there are verbs that explicitly label an instrument used in an event (such as "hammer", "mop", "harpoon") and because Clark (1981, 1982) and others have shown that some children will create innovative instrument verbs such as "brooming" and "pliersing" when these words are not in the adult lexicon (This occurs in languages other than English as well). I have found in earlier studies that while instrument verbs--appropriate or innovative--are rare in naturalistic speech, when children are asked to label events which have an appropriate instrument label as well as an appropriate action or result verb label, instrument verbs are used frequently, even by 3-year-olds. Instrument verb use also increases with age (Behrend, 1987a).

While studies of spontaneous verb use are instructive, they do not allow us to draw conclusions about default values of children's verb concepts. This is because there are many other factors, such as parental input, word frequency, and communicative context that may play important roles in determining what verb a child will use to label a given event at a given time. Also, when children label an event using a verb they alredy know, they are not relying on default values, but rather their existing verb concepts, in which action, result, and instrument slot values are already stored.

The strategy that I have used to study default values in verb concepts is to perform training studies in which children are taught novel verbs to label novel, videotaped events and then to test children's willingness to use these verbs to label additional events in which the instrument, action, or result is different from the events on which the verbs were trained. In the first study of this kind (Behrend, 1987b), children were taught six nonsense words. Each of the nonsense words was used to label three videotaped events in which an adult performed an unfamiliar action with a novel instrument that produced a clear result. After subjects were taught the novel verb, they were asked if they would use that verb to label additional events in which the instrument, action, or result was different from the events on which the verb was trained (the procedure will be described in detail when the main study of this paper is presented). It was found that all subjects were least willing to use a newly learned verb to label an event when the result was changed from the training events, followed by action and then instrument changes. A significant interaction showed that instrument changes were more important to preschoolers than adults and that result changes, though most important for all age groups, were least important for 3-year-olds. Action changes were moderately important for all age groups.

This experiment was an initial attempt at studying children's assumptions about verb meanings, and it raised many interesting questions. One important question is raised by the fact that in the training procedure that was used, all of the events on which a verb was trained were identical. Surely, this is not how verb learning occurs in the real world where various events can be labeled by the same verb. In fact, this variation between events is surely one of the factors that allows children to determine what, indeed, are the most crucial aspects of a particular verb's meaning and enables them to use the verb correctly. The study described in the remainder of this paper addresses this question by introducing systematic variations into the events used in the training procedure.

Method

Subjects

Twelve 3 year-olds (mean age 3;8) and 5-year-olds (mean age 5;4), and 12 adults (mean age 20;2) were subjects. There were eight boys and four girls in each group of preschoolers, and an equal number of adult males and females.

Materials

The stimuli were six sets of videotaped events each matched with a novel verb. The events and verbs used in the earlier study served as the basis for these events with some minor changes. For each verb, there were three training events. In all sets of training events, two of the three components (instrument, action, result) of the events remained constant while the other component was varied (the training variation). Thus, for two verbs the instrument varied; for two, the action varied; and for the remaining two, the result varied. Following the training events for each verb were 4 test events. One of the test events was identical to the first training event for that verb. In the other three test events, either the instrument, action, or result was changed (the test change). Table 1 shows three examples of the verbs and the

events that were used to teach them, one each in which the instrument, action, or result was varied in training. As there were two verbs used for each training variation, there were two trials for each of the possible training variation-test change combinations in the design. Thus, each subject made a total of 24 responses in the study. The test events on which no changes were made will not be discussed because subjects virtually always accepted the novel verb to label those events.

TABLE 1

Examples of novel verbs and events used in the training study.

---

1. **REMMING**: Twirling a ratchet / spaghetti server / bulb digger to collect yarn from a table.

Test Changes: I = Barbecue tongs A = Scooping R = Dangle yarn

2. **CHIFFING**: Sweeping / Spinning / Tapping tray with a wok cleaner to move popcorn to side of tray.

Test Changes: I = Teflon Brush A = Pushing R = Divide in 2 piles

3. **STIPING**: Reaching over a cup with a bike lock to flip it over. slide it towards you. / drag it on its side.

Test Changes: I = Rubber ring A = Inserting R = Crush cup

Procedure.

Before the first training event, the subject was told "Watch this person, she is remming." The first training event was then shown. Then, before the other two training events, the experimenter said, "Let's watch her do that again. Look, she's remming again." After the last training event, the subject was told, "O.K. Now I want you to tell me what she's doing. Look at her, and tell me if she is remming this time or if she's doing something else." The test event was shown, and the experimenter asked "Was she remming that time or was she doing something else?" This question format was used in order to avoid using a yes-no question. The order in which the novel verb and the phrase "something else" were heard was counterbalanced. After the last test event for a verb was shown, the training for the next verb began. Sessions lasted 15 to 20 minutes.

Results

The key dependent variable was the number of times subjects accepted the novel verb as a label for the test events. A 3 (Age) x 3 (Training Variation) x 3 (Test Change) ANOVA was used to analyze the data with the last two factors being within-subject factors. Table 2 summarizes the data. It should be kept in mind that lower values in this table represent stronger effects (i.e. subjects were less willing to accept the novel verb to label the test events). The significant main effect for test change,

$F(2,66) = 54.3$, $p<.001$, showed that, as in the earlier study, result changes had the strongest negative effect on subjects' acceptance of the novel verb, followed by action and instrument changes. There was also an age x test change interaction, $F(4,66) = 2.74$, $p<.05$, which showed that instrument changes were more important and result changes were less important to 3 year-olds than to either of the other age groups. Overall, the subjects in this study were more likely to accept the novel verb as a label for the test events than subjects in the earlier study. This was a direct effect of the variations in training that were introduced in the current study.

TABLE 2

Number of times novel verb is accepted as a label for test events (maximum value = 6)

	Age			
Test Change	Three	Five	Adult	Overall
Instrument	4.6	5.3	5.3	5.1
Action	3.3	2.6	3.3	3.1
Result	2.8	2.1	2.3	2.4
Mean	3.5	3.3	3.6	3.5

Of great interest in this study was precisely how the traing variations affected subjects' use of the novel verb and interacted with the test event changes. It was expected that if subjects were sensitive to the variations in the training, and, all other things being equal, then subjects would be more willing to accept the novel verb for a test event in which the aspect that was changed was also the aspect that was varied in the training for that verb. For example, subjects should be more willing to accept the novel verb as a label for the test events in which the action was changed when the actions were varied in the training events than when instruments or results were varied. These specific predictions were tested with a set of orthogonal planned comparisons based on the overall ANOVA.

Many of these predictions were confirmed. Figure 1 displays the significant interaction between training variation and test change, $F(4,132) = 24.7$, $p<.001$. It can be seen from the top panel in Figure 1 that when instruments were varied in training, the novel verb is no more likely to be accepted as a label for the test event in which the instrument is changed again than in the other two training conditions. This is primarily because instrument changes do not have much of an effect on use of the novel verb in the first place. However, the predicted effects for the action and result training variations were observed. That is, the novel verb was more likely to be accepted for action change test events when actions varied in the training events, $F(1,35) = 61.2$, $p<.01$, and it was more likely to be accepted for the result change test events when results varied in training compared to the other two training conditions, $F(1,35) = 7.17$, $p<.01$.

In addition, there was a 3-way interaction between age, training variation, and test change $F(8,132) = 2.78$, $p<.01$. Looking at the data for each age group, it was found that the findings for the instrument and action training variations held across age groups. That is, there was no effect for any age group for instrument variations, and there was a significant effect for all age groups for action variations. However,

FIGURE 1: DATA FOR ALL SUBJECTS

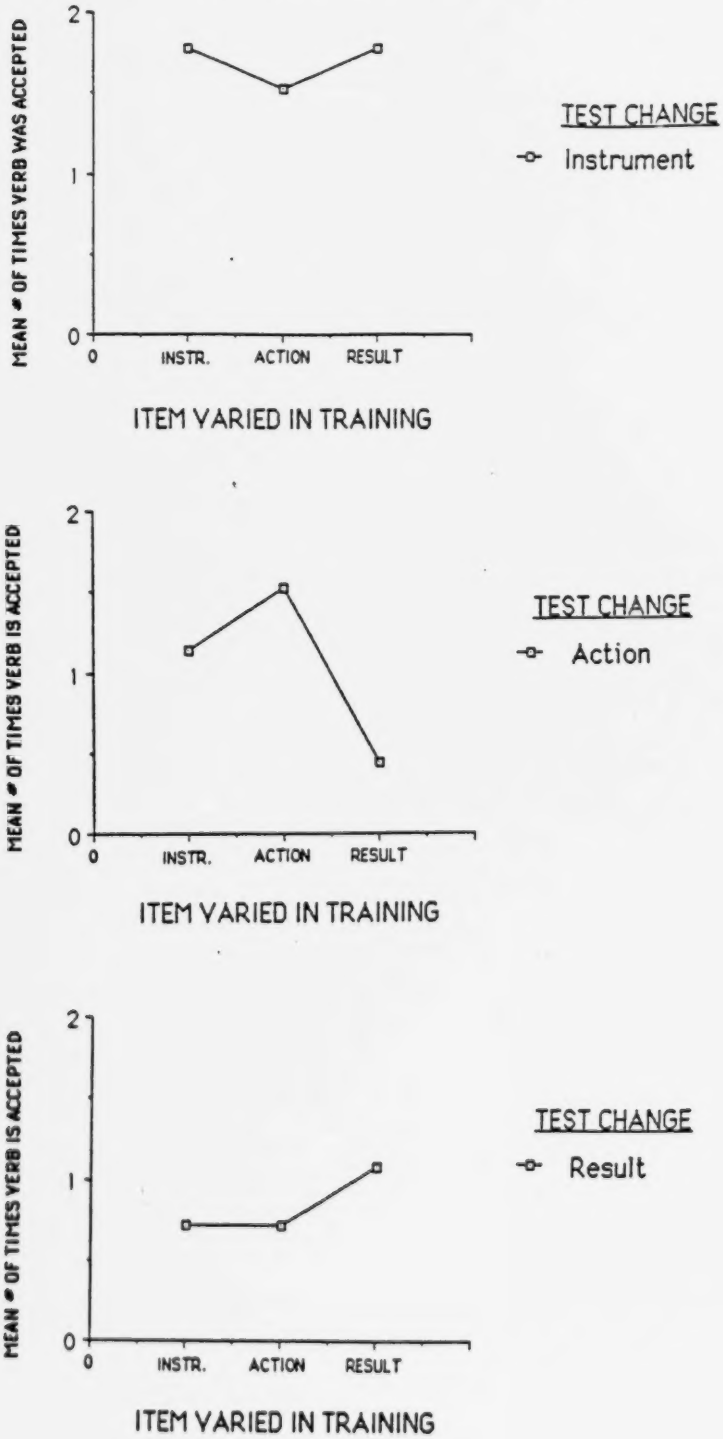
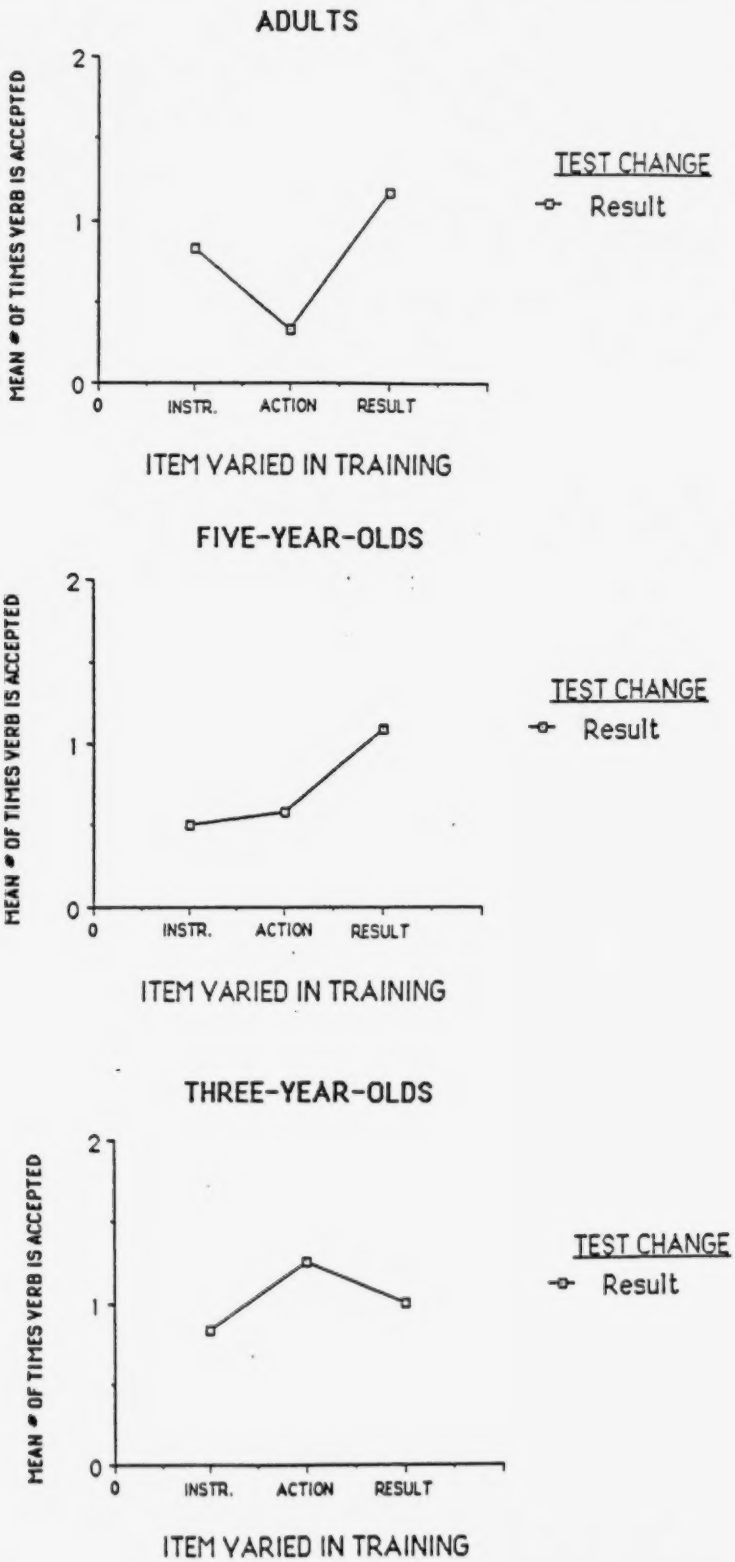


FIGURE 2: RESPONSES TO RESULT TEST CHANGES



varying the result in training had differential effects on the three age groups. Figure 2 shows that while there was no effect for the three year-olds, the predicted effect was seen somewhat in the five-year-olds, $F(1,11) = 4.96$, $p=.048$, and clearly in the adults, $F(1,11)=8.43$, $p<.03$.

In summary, when actions were varied in training, all age groups were more likely to accept the novel verb as a label for an event in which the action was changed again. When results were varied, only adults and five-year-olds were more likely to accept the verb as a label for a test event in which the result was changed again. Varying instruments in the training events had no effect on the use of the novel verb for any age group.

There is one other finding that is worthy of attention. Returning to the second graph in Figure 1, it can be seen that when results were manipulated in training, action changes had a profound negative effect across age groups on subjects' willingness to use the novel verb. What this finding suggests is that when results are varied and, thus, reduced in importance in the verb's meaning, perhaps some subjects abandoned the hypothesis that results are most important, and switched to action as the component most likely to be central to a novel verb's meaning. This was an unexpected finding and one deserving of further investigation.

Discussion

A picture is now beginning to emerge about children's initial mapping of verb meanings. When all other things are equal, it appears that the default assumption is that a novel verb is a result verb, as shown by children's unwillingness to use a newly learned verb to label an event in which the result was changed. This bias appears to increase with age. Actions are assumed to be less important than results, and instruments are rarely assumed to be important to novel verb meanings, though instruments were somewhat more important to 3-year-olds than to 5-year-olds and adults.

In addition, this study showed that some intriguing and potentially important things happen when children's default assumptions are in conflict with the input that is received about a verb's meaning. This was essentially what happened when results were varied in training in the present study, given that the assumption is that a novel verb is a result verb. It was found that when the result of an event was varied in training, adults, and to a lesser degree 5-year-olds, showed the predicted pattern of more frequently accepting the novel verb for an event in which the result was changed again. Some 5 year-olds, and to a much greater extent 3 year-olds, were apparently unable to override their default assumptions that the result is the most important component in a novel verb's meaning and frequently rejected the novel verb as an appropriate label for the test events in which the result was changed again. This is potentially an important developmental difference in the initial mapping of a verb meaning and merits further study.

It was also found that when results were varied in training, subjects of all ages rarely accepted the novel verb for the test event in which the action was changed. This finding suggests that there may be a ranking of sorts among the slots in verb concepts such that if results are decreased or eliminated in importance in a verb's meaning, then it is hypothesized that action, instead, is the key to the meaning of a novel verb. This account is similar to a "default rule hierarchy" that has been proposed to account for various types of animal, human, and machine learning (e.g. Holland, Holyoak, Nisbett, & Thagard, 1986) and demands consideration as the mechanism responsible for this finding of the present study.

Conclusion

To conclude, fast mapping appears to be a useful way to characterize the learning of verb meanings. However, it also appears that the hypotheses that guide children's initial mappings (i.e. their default values) are still changing in the preschool years and that there may be important and (perhaps) hierarchical relationships between default values in different slots in verb concepts. Additional research with younger children and a wider range of verbs will help to clarify the exact nature of the sources of these changes and the processes involved in the initial mapping of verb meanings.

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Evidence for the VP Constituent from Child Korean*

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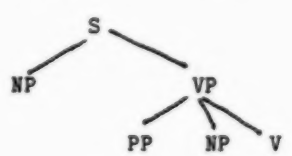
0. Recently many interesting discussions (Hale 1982, Mohanan 1982, Saito 1985, Whitman 1979, 1986) have centered around a typological distinction between configurational and nonconfigurational languages. Loosely defined, configurational languages are those in which word order is fixed, whereas scrambling type free word order is observed in nonconfigurational languages. Korean and Japanese have been identified as nonconfigurational, due to their relatively free word order. Consider the following:

(1) a. nay-ka Suni-eykey sakwa-lul cwu-ess-ta.
I-NOM DAT apple-ACC give-PAST-VE
'I gave an apple to Suni.'
b. nay-ka sakwa-lul Suni-eykey cwu-ess-ta.
c. Suni-eykey nay-ka sakwa-lul cwu-ess-ta.
d. Suni-eykey sakwa-lul nay-ka cwu-ess-ta.
e. sakwa-lul nay-ka Suni-eykey cwu-ess-ta.
f. sakwa-lul Suni-eykey nay-ka cwu-ess-ta.

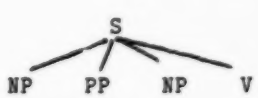
As demonstrated in (1), the words in a sentence can occur in any order, with the exception of the verb, which should be rigidly final.

1. The freedom in word order leads to an important question regarding a verb phrase constituent. A simple generalization which we can make from (1) is that only the sister constituents directly dominated by S are freely ordered. Due to this fact, recent studies of Korean question the existence of a VP constituent. Consider (2):

(2) a.



b.



If we assume (2a) to be the constituent structure of the sentences in (1), there is a VP constituent and the first NP is not a sister node of the PP or of the second NP. Accordingly, it is difficult to explain why scrambling is possible without relying on other factors such as different representation levels or movement. In contrast, if we assume a flat structure lacking a VP constituent as in (2b), all words are in the sister relationship and can be freely scrambled.

2. However, a careful look at the whole structure of Korean reveals that this issue of presence or absence of VP is more complicated. We can find various independent evidence not only for a VP constituent but also against it, as we will see shortly. On the one hand, there are studies (Hale 1982, Saito 1985, Choe 1985, etc.) that claim that nonconfigurational languages have a VP constituent at

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least at a certain level. Hale proposes that both configurational and nonconfigurational languages have a VP constituent at the level of Lexical structure, whereas the latter lacks VP at Phonological structure. In contrast, Saito advances arguments for the position that nonconfigurational languages have VP at every level and that scrambling is the result of syntactic movement. In this paper, we will not deal with the differences between the two approaches. On the other hand, there are studies (Hinds 1974, Mohanan 1982 for Malayalam, etc.) that assume a flat constituent structure on the basis of the lack of any positive evidence for VP.

3. First, let us consider arguments for VP. As observed in (2a), positing the VP constituent results in distinguishing between subjects and non-subjects. Accordingly, any syntactic phenomenon referring to the subject and non-subject asymmetry constitutes an argument for VP. Saito's and Whitman's arguments that the distribution of PRO is restricted to subject position in Japanese seem equally applicable to Korean as shown in (3).

(3) a. ttena-to coh-ta.
leave-even good-VE
'It is good even if (PRO) leaves.'
b. ney-ka poa-to coh-ta.
you-NOM see-even good-VE
'It is good even if you see (it).

In (3), only unexpressed subjects may be interpreted as arbitrary reference, while unexpressed non-subjects have to be interpreted as coreferent to something mentioned in prior discourse. In other words, PRO may occur only in the subject position. Due to the nature of PRO which requires it to appear in an ungoverned position, we need VP which governs non-subject position but not subject position. The other strong argument refers to the binding condition C, stating that r-expressions must be free (Whitman 1986).

(4) Minsu-ui chinkwu-ka ku-lul ttayli-ess-ta.
GEN friend-NOM he-ACC hit-PAST-VE
'Minsu's friend hit him.'

Only if we assume VP, can we account for (4). In (4), the pronoun does not c-command an r-expression since VP, a maximal projection, blocks c-commanding. Hence the r-expression is free. A flat structure lacking VP incorrectly predicts that (4) is ungrammatical since the pronominal object would c-command the r-expression.

4. Let us now consider some arguments against VP, most of which are based on negative evidence. According to Hinds (1974) and Whitman (1986), Korean or Japanese does not give any evidence for rules referring to a VP constituent, such as VP movement or VP deletion rules. In the case of VP deletion, Korean has a verbal construction which corresponds to "do so" pronominalization in English as in (5).

(5) Suni-nun hakkyo-ey ka-ass-ta. Minsu-to kuletkey ha-yess-ta.
TP school-to go-PAST_VE too thus do-PAST-VE
'Suni went to school, and Minsu did so too.'

However, non-anaphoric direct objects may be freely retained in Korean, while English requires that the whole VP constituent including them should be replaced as in (6).

(6) Suni-nun TV-lul Youngmee-eykey cwu-ess-ta. Minsu-nun stereo-lul
TP ACC DAT give-PAST-VE TP ACC
kuletkey ha-yess-ta.
thus do-PAST-VE
'*Suni gave the TV to Youngmee, and Minsu did so the stereo.'

Case assignment phenomenon provides another piece evidence against VP (Whitman 1986). According to Schwartz (1972), adverbs may not normally intervene between verb and direct object in English. In contrast, in Korean, adverbs and indirect objects may be placed between verb and direct object. In both cases, the direct object is clearly case marked. This phenomenon is accounted for most readily if we conclude that case assignment is not restricted by adjacency in Korean. The absence of an adjacency requirement for case assignment leads to the assumption that case in Korean is in general inherent. Then what does the absence of structural case show regarding the VP constituent? If we consider structural case to be assigned by V only when the NP is a daughter of VP in English, we do not have to posit the VP node for case assignment in Korean. If these observations are right, positing the VP node in Korean would unnecessarily complicate the grammar, obscuring possibly simpler analyses.

5. As shown above, the debate concerning the presence of the VP node has not been resolved yet for adult grammar. The positive and negative evidence seems to be evenly divided. In this context, we propose one piece of positive evidence for VP in child grammar. In the next section, we will show why it is necessary to posit the VP node in child Korean by examining children's systematic errors with the negative construction.

Korean has two negative constructions. One is what is often called "short form" or "pre-verbal" negation and the other is the "long-form" or "post-verbal" negation. The following sentences illustrate the differences.

```
(7)  a. Suni-ka     ka-ass  ta.
             NOM    go-past  VE
        'Suni went.'
     b. Suni-ka      an     ka-ass   ta.
             NOM     Neg    go-past  VE
        'Suni did not go.'
     c. Suni-ka     ka-ci   anh-ass          ta.
             NOM    go-COMP NEG.verb-past    VE
        'Suni did not go.'
```

Whether or not there are any semantic differences between the two types has been the source of some debate in the past (Cho 1975 , Kuno 1980, Song 1982). Even though there seem to be speakers who draw a subtle distinction between the two, we will treat the two forms as synonymous except for a stylistic difference. The main syntactic difference between (7b) and (7c) is that the negative particle *an* precedes the verb *ka* ("to go") in (b) whereas in (c) a negative auxiliary verb is employed, preceded by an inflected form of the main verb. It is quite obvious that the post-verbal negation shown in (c) is more complex and is acquired much later by children. A longitudinal study by Choi and Zubin (1985) reveals that the preverbal form appears at around the age of 1;9 while the post-verbal form does not occur until about age 3:5.

What is interesting emerges in an elicitation study. When given sentences involving the post-verbal negation for repetition, three 2-year olds we have investigated invariably substitute the simpler pre-verbal counterparts for the post-verbal sentences. (8) is one such example.

```
(8)   Adult:namu-e   ollaka-ci  anh-a
            tree-Loc  climb-Com Neg-VE (post-verbal Negative)
          "(He) does not climb trees."
      Child:namu-ui-e an ollak-a
            tree-Loc  Neg climb-VE (pre-verbal negative)
          "(He) does not climb trees."
```

This substitution confirms the idea proposed by Slobin (1985) that "a separate rather than a bound morpheme" is preferred for clausal negation. Therefore, children acquiring English "

often mov[e] the negative operator outside of the verb complex or clause", as illustrated in such examples as *No do this* or *I no do* this. Even in languages where there is no separate negative particle such as Hungarian, Polish and Turkish, children often use a free morpheme instead of the correct bound inflected form. In Korean, where both constructions are possible, it is only natural that children should prefer the pre-verbal negation. In short, our 2 year olds are not yet ready to produce the post-verbal negation even for repetition even though all of them seem to extract the negative meaning out of the long form negative, thus confirming the widely accepted idea that comprehension precedes production in language acquisition.

What is directly relevant for our discussion in this paper is the preverbal negation in which the negative particle *an* or *mos* is placed immediately before the verb in adult grammar as shown in (7) b. Our three youngest subjects manifest a strikingly different pattern in their spontaneous speech behavior in contrast to our older subject(2;10). Our four subjects are represented in (9). For each child, we collected one hour of speech in a natural setting, that is, recordings of conversations between the mother and child (sometimes with the investigator) in the home. Each session was tape-recorded and transcribed incorporating a written record made at the time of the recording in order to clarify the meaning of the interchange. Also we provided supplementary data by performing an experiment with each child involving the elicitation of negative constructions.

(9)

S	2;4	MLU	1.68
J	2;2	MLU	2.39
M	2;6	MLU	3.85
G	2;10	MLU	4.83

The younger children, in contrast to our oldest subject, predominantly place the negative particle before the whole verb phrase rather than in the pre-verbal position as dictated by adult grammar.The relevant data are shown in (10).

(10) a. hyengsuni an ca
hyengsuni NEG sleep ('Hyengsun does not sleep.')

na an ttaliye
I NEG hit ('I do not hit (him).')

nwun an poye (passive)
eye NEG see-passive ('The eyes are not visible.')

ike an thulieye (passive)
this NEG turn-on-passive ('This cannot be turned on.')

ecumma mos poye(passive)
aunt NEG see-passive ('Aunt is not visible.')

Babie-un an ssappun ayki-ye
TOPIC NEG bad baby-be ('Babie is not a bad baby.')

b. na an mek-e
I NEG rice eat-NE ('I do not eat rice.')

```
khoch-i    an   nolay  pwulie
flower-NOM NEG  song   sing  ('The flowers do not sing a song.')

Soyeni-nun   an  son  takk-ko siphinna
      TOPIC NEG hand wash    want-to
                        ('Soyen does not want to wash hands.')

an  mamma mantul-e
NEG meal  make    ('(I) do not make meals.')

an chilul coa-hay
Neg pickle like  ('(I) do not like pickles.')

an  chong ssso-ss-e
NEG gun   fire-PAST   ('(I) did not fire the gun.')

an  uyuu ssot-ass-e
NEG milk spill-PAST  ('(I) did not spill milk.')

c. an  cal  hay
   NEG well do     ('(I) do not do well.')

   an  manhi huly-ess-e
   NEG many  draw-PAST    ('(I) did not draw many pictures.)

   mos cal  tha
   NEG well ride    ('(I) do not ride (a horse) well.')

   an  mak ulo
   NEG much cry     ('(I) do not cry much.')

   na an  cal  hay
   I  NEG well do   ('I do not do well.')

d. an  Seoco  ka
   NEG        go    ('(I) do not go to Seoco.')

   an  yekise hay
   NEG here   do    ('(I) do not do (that) here.')

e. an  way an  hay
   you why  NEG do  ('Why don't you do (that)?')
```

(10a) represents a case where the negative an is placed between the subject and the verb as it should be in the adult grammar. We observe that children place an always after the subject, if there is one, both in an active or passive sentence. This suggests that the negative placement is sensitive to grammatical functions rather than to thematic roles. (10b) shows cases where the negative is placed before an object. (10c) and (d) demonstrate its placement in relation to verbal adverbials like manner and place adverbs. These examples show that the negative particle is placed after the subject, but always before the elements that are conventionally regarded as belonging to

the verb phrase.

6. At this point, several possible analyses can be advanced, one of which is to say that the negative particle is placed after the subject, rather than before the VP. The other possibility is to claim that what is relevant is not syntactic categories but some discourse notions such as topic and comment. These two alternative analyses can be rejected on the basis of the sentence shown in (10 e). (10 e) is an example that involves a sentential adverb *way* ("why"). We observe that the child places the negative particle after the sentential adverb if there is one. This immediately rejects an analysis that views grammatical functions as a relevant factor. Therefore, "after the subject, *if* there is one" cannot be the right solution. Also, any analysis that regards the topic/comment distinction as a solution and places the negative particle after the topic rather than after the subject will be disconfirmed. It incorrectly predicts that the negative particle should precede the adverb 'why', which is new information and cannot be the topic of the sentence. What we find in (10 e) is that the negative morpheme is placed after the subject and after the sentential adverb.

Then we can conclude that an analysis based on phrase structure rules provides a simpler account. Now we can approach the question of the existence of the verb phrase in the child grammar.

If we assume a VP node, then a very simple rule, in (11) can account for the data.

(11) S---->(NP) (S-Adv) Neg VP

All that the child needs to know with regard to the negative placement is one simple rule of placing it before the VP. Then it automatically follows from (11) that *Neg* always precedes any verbal argument but follows anything else in the sentence.

On the other hand, if we do not assume a VP node, we would have an arbitrary phrase structure rule like the following.

(12) S---->(NP)(S-Adv) Neg (NP)(PP)(Adv) V

Even if the phrase structure rule can be formulated as in (12), we find that it is not sufficient. It is necessary to stipulate that the first NP has to be the subject. The child has to know not only the linear order among the syntactic categories involved but also the grammatical function each category bears. In addition, no explanation can be given for why the child would place the Negative in that particular place, namely, after the subject and after the sentential adverb, as opposed to any other position in the sentence. Since there seems to be no constituent which comprises the subject and the sentential adverb but not the others, the negative could be placed in any position, which is very arbitrary and unrevealing.

Having argued for the VP constituent in child grammar, we would like to put forward some speculations as to why the child consistently places the preverbal negative marker before VP instead of any other possible slots in the sentence. For instance, why does the child not place the marker in the sentence initial position?

Slobin (1985) shows that cross-linguistically children indicate in their restructuring of parental language that the scope of negation should be the proposition, as indicated by the verb or the clause as a whole, rather than any particular nonverbal lexical item within a clause. This universal tendency to put the negative in its logical scope in such a way to negate the whole proposition also seems to play a role in acquiring Korean.

Also relevant is the fact that in Korean "before the VP" is the unmarked place to put elements with sentential scope. Consider the following.

(13) a. ne-nun eney eoseney-kkaci cal ha-yass-ni?
2-TP yesterday how.much-all well do-PAST-Q
'I did however well yesterday.'

b. na-nun ecey cal [illegible]y-lul ha-yess-ta.
I-TP yesterday well [illegible]rk-ACC do-PAST-VE
c. ecey na-nun [illegible]-lul cal ha-yess-ta.
yesterday I-TP homework-ACC well do-PAST-VE

As shown in (13), a sentential adverb like *ecey* ("yesterday") is placed before VP, while a verbal adverb like *cal* ("well") is placed in the pre-verbal position in the unmarked case. As in (13b), verbal adverbs cannot be placed before VP. Even though (13c), where the sentential adverb is placed before the whole clause, is also grammatical, there is a subtle distinction between (13a) and (13c). (13c) is in most cases used to put some emphasis on the sentential adverb *ecey*. Accordingly the sentence may imply some contrast such as *yesterday, but not the other days*. (13a), which we believe to be the unmarked order, does not denote such contrastive meaning. In sum, (13) clearly shows that sentential adverbs are placed before VP in the unmarked case. If this generalization is right, the negative particle *an*, the scope of which is relevant to the proposition to the child, is naturally placed before a VP constituent until the child learns the right word order.

7. In the above discussion, we have argued that we need a VP constituent in order to capture the right generalization. Then the next question that arises is: how does this fact in child grammar bear on adult grammar? As shown in the first part of the paper, arguments for VP and those against VP in the adult grammar are almost evenly divided, so that the presence or the absence of VP cannot be easily established. In the midst of this controversy, can we use the fact that Child Korean has the VP constituent as an argument for the same constituent in the adult language? We think not. If extensive reorganization takes place in the course of language acquisition such that the initial analyses the child makes bear little resemblance to the adult knowledge (Schlesinger 1967, Bowerman 1974,1977, de Villiers et al., 1977), a fact about child language does not directly constitute evidence for adult language. However, we can safely assume that language acquisition is a process that is conservative, so that not any random revision is possible and that the potential for reorganization in the child's grammar is minimal, i.e. the child abandons the linguistic entities only when confronted with positive evidence against his initial hypotheses. If this hypothesis proves to be right, then this leads to an interesting prediction. If it turns out by independent evidence that there is no VP in the adult system, then claims can be made that syntactic categories such as VP are provided by Universal Grammar and that, depending on the kind of input language that the child receives, certain entities, though present in the child grammar, can be overridden as the child realizes those entities are not used in the adult system. Pinker's generalization (1984) that the child may very well restrict himself to a subset of the attested orders when a language has free constituent order sheds light on this. Korean children also use the SOV order most of the time: they hardly ever use OSV order, despite the occurrences of the SOV, OSV orders in parental speech. When they use only the SOV order, a VP constituent is consistent with the word order in their grammar, since the object is closer to the verb than the subject is on the configurational structure. However, as they acquire various word order possibilities and scope relations, they may realize that the concept of a VP constituent is no longer consistent with the word order facts of the adult language and thus abandon it.

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THE ROLE OF STRESS, POSITION AND INTONATION IN THE REPRESENTATION AND IDENTIFICATION OF EARLY WORDS

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The question of how children initially extract and represent approximately word-sized units from the stream of adult speech is one of the most fundamental of language acquisition. Unless the child can identify word-level units, virtually all linguistic accomplishments will be impossible. It is also a far more difficult problem than it may initially appear: Contrary to adult intuitions, words are rarely separated by pauses and there are generally few, if any, consistent cues to boundaries between words (Cole & Jakamik, 1980; Hayes & Clark, 1970).

It has been suggested that certain attentional biases of the child may limit the difficulty of this task. Rather than attending to and attempting to segment an entire sequence of adult speech, the very young language learner may be predisposed to focus primarily on certain elements of that speech sequence. Those elements would tend to be extracted and should be prominent in the child's early representations and productions.

The notion that certain elements of the input speech are particularly salient to young language learners was originally proposed by Slobin (1973) in the context of the acquisition of morphology. On the basis of cross-linguistic data on the order of acquisition of various morphological inflections, Slobin argued, for example, that children pay particular attention to elements at the ends of utterances. More recently, perceptual biases have also been discussed in the context of segmentation. Thus, for example, Gleitman and Wanner (1982) have argued that a tendency to attend to and extract stressed syllables will account for the initial segmentation of speech.

If children are, in fact, biased to attend to stressed and final syllables and if those biases do assist the child in identifying approximately word-level units in the stream of speech, then those perceptual biases should be reflected in first utterances. More specifically, first utterances should tend to contain primarily stressed and final syllables. In fact, stress and position do appear to account for many first utterances. Thus, for example, children may produce /čak/ for "chocolate," /lai/ for "butterfly," or /rai-sə/ for "eraser," each of which contain only stressed, final or stressed and final syllables.

Further support for this position comes from experimental and descriptive studies of children's imitations, as well as some crosslinguistic data. Children will tend to include stressed or final syllables in their productions even for imitations of nonsense-words (Blasdell & Jensen, 1970; Frumhoff & Newport, in preparation) and for languages in which stress falls on inflectional morphemes rather than on the content words of the language (Fueur, 1980; Pye, 1983).

An important test of the claim that the proposed biases can account for the nature of first words could be provided by a systematic analysis of a corpus of early utterances. I will, therefore, give a brief description of a study which was intended to do just that. As will be seen, however, biases to attend to stressed and final syllables, while accounting for a great many first utterances, can by no means account for all. It has, in fact, been argued, that children may extract, as single units, longer sequences of speech which are defined by intonation or rhythm (Peters, 1977, 1983). Furthermore, although children's productions may be suggestive as to which elements of speech are perceived as most salient, productions are, of course, not

necessarily identical to representations. With respect to the former of these concerns, the second study, and that which is the primary focus of this paper, investigated the possibility that prosodic properties may be important in the early extraction and representation of word-like elements of speech. In the interest of assessing children's representations more directly, the data for this study were children's recognitions rather than their productions.

Analysis of Early Utterances

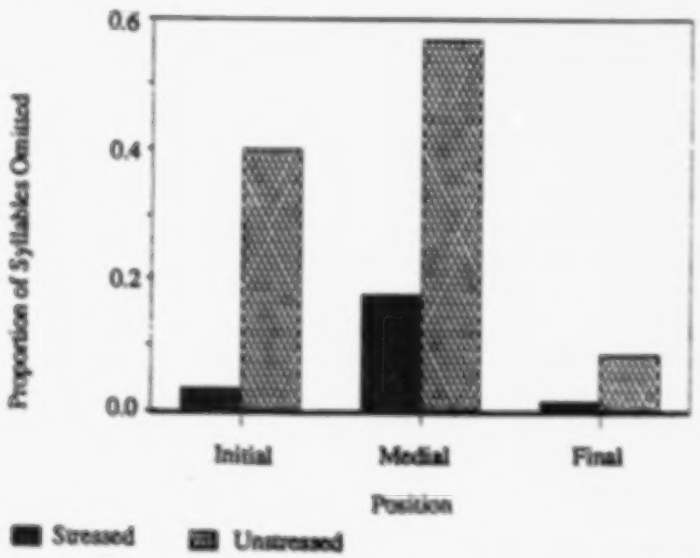
The corpus submitted to analysis was collected from three children between the ages of 17 and 23 months, each of whom was producing primarily single-word utterances. Children were tape-recorded in their home during natural interactions between the child and parent or child and experimenter. Utterances with adult targets longer than a single syllable (a total of 615 utterances) were phonetically transcribed and coded in various ways. For present purposes, certain syllable-level codings are most relevant: Each syllable of the child's utterance was coded, in relation to the corresponding syllable of the adult target, for presence or absence and for accuracy.

Two primary predictions were tested. The first was that syllables which were unstressed and nonfinal should more frequently be omitted than stressed or final syllables. Secondly, because stressed and final syllables are particularly salient, they should be more completely extracted. As a result, where unstressed syllables are included in a child's production, they should be less accurately produced than stressed or final syllables.

Both predictions were borne out. Figure 1 shows the proportion of syllables omitted as a function of stress and position. As can be seen, syllables which were both unstressed and

Figure 1

Proportion of Syllables Omitted as a Function of Position and Stress

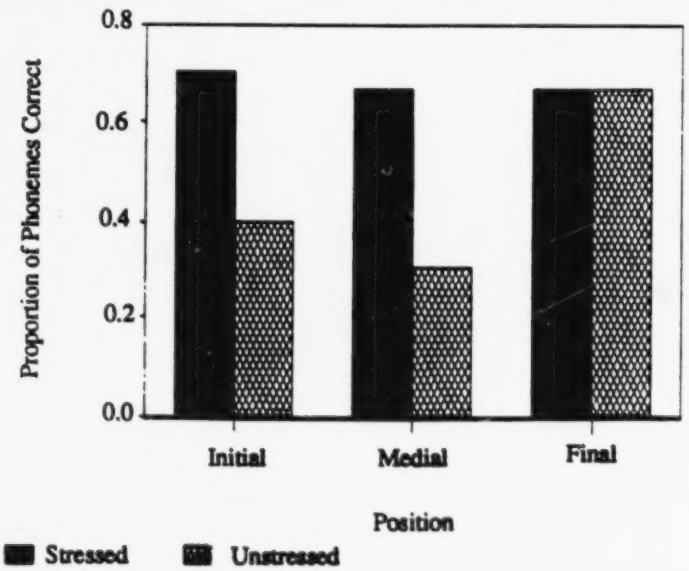


nonfinal were omitted far more frequently than stressed or final syllables. In Figure 2, the

proportion of phonemes correct is shown as a function of the stress level and position of the target syllable. Syllables for which the target was stressed or final were also produced more accurately

Figure 2

Proportion of Phonemes Correct as a Function of Position and Stress



than unstressed or nonfinal syllables. These results are consistent with the position that young language learners are particularly likely to focus on and extract stressed and final syllables.

Children do not, however, extract only stressed and final syllables. Children will frequently produce utterances containing filler or "dummy" syllables, in which syllables of the adult target have been reduced to a schwa or other undifferentiated sound (e.g., /m-bɛ-dɛ/ for "all-better" or /ʌ-tat/ for "what's that"). These filler syllables may frequently correspond to target syllables which are neither stressed nor final. In such cases, the child has clearly stored some representation for the unstressed, nonfinal syllable but it is a representation which appears to lack the phonemes of the target syllable. Children also produce utterances which include only some of the phonemes of the adult target, and which appear to be defined primarily by the stress pattern or intonation of the adult target (e.g., /da-dʌ-do/ for "here-we-go," /hi-i-ya/ for "here-they-are"). I will describe such utterances as "underanalyzed sequences," although they have also been described by Peters (who identified the phenomenon in a 1977 paper) as "tunes" and "gestalt" speech. Filler syllables and underanalyzed sequences make evident the need for something more than only biases to attend to stressed and final syllables in a model of first utterances.

Turning back to stress for a moment, the research described earlier indicates that stress has an effect on what is extracted. The existence of these other types of utterances suggest another potential role for stress: A child may extract the stress pattern, itself, or stress as it contributes to the overall intonation contour or rhythm of an utterance. If a perspective is taken within which the child may extract a sequence defined by rhythm, intonation or stress independently of the

phonemes, then filler syllables could, for example, be described as the extraction of a rhythmic syllable without the associated phonemes. Underanalyzed sequences would then be described as the extraction of the stress or pitch pattern of a word or phrase, with only a few of the associated phonemes.

An extreme test of the view that children may attend to the stress *pattern*, and not only to stressed syllables, would be to construct a situation in which children must choose between stress pattern and phoneme sequence as the basis for identifying the referent for a newly learned label. In that way, it would be possible to assess whether, in some cases, the stress pattern may be more prominent in the child's representation of a word than the segmentals. This second study was designed for that purpose. It should be mentioned here, however, that although I would like to argue that it can be intonation, or one of several components of the prosodic contour of a speech sequence, that children will attend to and extract, the study described here tests for a tendency to attend to only one such component: the stress pattern. Additional research will be required to determine which components of an overall intonation contour a child may attend to, extract, and store as part of the representation for a word.

Method

Subjects. Subjects were 32 two-year olds and 32 three-year olds, from five daycare centers located in several Midwestern communities. Half of the children in each age group were assigned to an experimental condition and half to a control. In addition, 16 adults, undergraduates at the University of Illinois, participated in the experimental condition.

Design and Procedure. Children were taught names for two objects, one at a time. The names were presented in carrier sentences, such as, "that's a /buta*ne*/" (where italics indicate primary stress). The child was asked to repeat the object names and was then presented with both objects and tested to be sure he or she knew the names.[1] Once the child had twice successfully identified each object (in response to "where's the .."), the child was presented with the test stimulus--a word which carried the stress pattern of one of the trained labels but the segmental sequence of the other. The child was asked, as before, to choose the object corresponding to that name. For example, a child who had been taught /buta*ne*/ for one object and /wo*lɛ*zi/ for the second object, might be asked "where's the /wo*lɛ*zi/?" The dependent measure was whether the subject chose the object that had previously been labelled with the intonationally or the segmentally similar name. Responses in which the child chose the object which had been associated with the intonationally similar label were coded as "intonational" while those in which the choice was the object associated with the segmentally similar label were coded as "segmental." A total of three sets of two object labels, varying in the degree of similarity between the two words of each pair, were taught to each subject. The word pairs and stress patterns are shown in Table 1. The stress pattern assigned to each word, and the particular word serving as the test, were fully counterbalanced between subjects.

[1] Although it would have been preferable, for consistency purposes, to have the labelling taught by a tape, piloting indicated that children could not learn the words from the tape. Object names were, therefore, taught by an experimenter. Acoustic comparisons of a subset of the stimulus utterances are being carried out to assure that there are no differences in the stimuli presented to subjects of different ages or in different conditions. Although those analyses are not completed, preliminary comparisons suggest that there are no important differences.

Table 1

Wordsets and Stress Patterns for Study 2

Wordset	Wordpair	Stress Pattern
Wordset 1	kibamu gidanu	0X0 X00
Wordset 2	togufa dopæ sa	X00 00X
Wordset 3	wolæzi butane	00X 0X0

Note. X = primary stress.

The procedure for children in the control condition was the same as that for those in the experimental condition except that the pair of words heard by each child did not differ in intonation, and the test word was identical to one of the trained words. The word pairs and contours were those used in the experimental condition and were, again, fully counterbalanced between subjects.

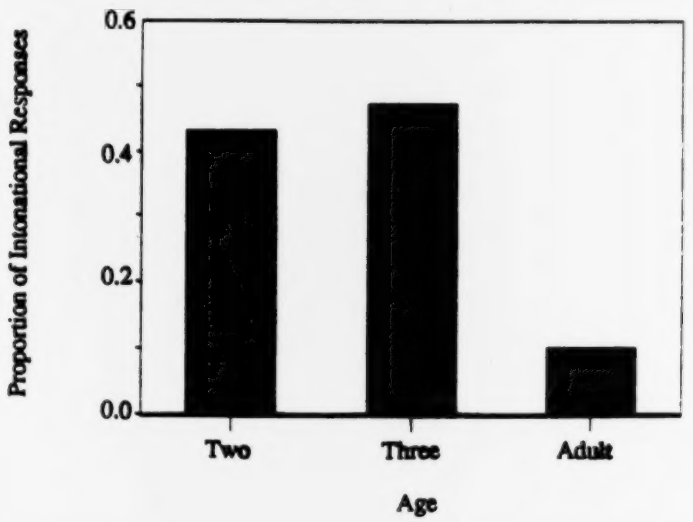
The proportion of "incorrect" responses obtained in the control condition served as a baseline to which the proportion of "intonational" responses obtained in the experimental condition could be compared. The justification for this is that incorrect responses reflect instances in which, in the absence of conflicting information, the child makes a "non-segmental" response. It was necessary to use such a baseline for the comparison, rather than comparing the proportion of intonational responses to chance (50%), because the claim is not that children will *always* attend to intonation rather than phoneme sequence. My previous work clearly shows that they frequently do attend to and extract a stressed or final syllable and that those syllables are frequently analyzed quite accurately. What I want to argue is that the stress pattern or other intonational characteristics may, in some cases, be extracted in lieu of, or along with, the phonemes of the stressed and final syllables and that these intonational characteristics will more frequently be prominent in the child's representation of a word than in the adult's. As a result, the stress pattern or intonation may sometimes override the segmental characteristics of a word in determining the child's perception of similarity between words. For adults, in contrast, the phoneme sequence should be far more prominent in the representation of a word than the stress pattern. Thus, adults should virtually always perceive similarity on the basis of segmental similarity rather than intonational similarity.

Results. A score reflecting the number of intonational responses was calculated for each child. Figure 3 shows the proportion of intonational responses for experimental subjects in each of the three age groups. Because each of the three wordsets were different in nature, the validity of a partial score was questionable. Any subjects who had produced one or more uncodable responses were therefore omitted from the analysis. (Two three-year olds and four two-year olds were omitted from analyses for this reason.) Chi-square analyses revealed a difference between children and adults in proportion of intonational responses, with children choosing the object associated with the intonationally similar label far more frequently than adults, $\chi^2(3) = 17.4$, $p < .001$. Two-year olds did not, however, differ from three-year olds, $\chi^2(3) = 1.37$, $p > .25$.

To determine the meaningfulness of these results, it is necessary to compare the pattern of intonational responses to the baseline obtained from children participating in the control

Figure 3

Proportion of Intonational Responses Across Three Age Levels



condition. Table 2 permits comparisons of the proportion of intonational responses for children and adults in the experimental condition and the proportion of incorrect responses for children in the control condition. Two-year olds, in particular, show fairly high error rates. As a result,

Table 2

Proportion of Intonational/Incorrect Responses as a Function of Age and Condition

	Age		
Condition	Two	Three	Adult
Experimental	.43	.47	.10
Control	.31	.23	---

Note. Scores for Experimental condition reflect intonational responses; those for Control condition reflect incorrect responses.

although the proportion of intonational responses produced by three-year olds was significantly different from the proportion of errors produced in the control condition, $\chi^2(3) = 9.31, p < .05$, this was not true for two-year olds, $\chi^2(3) = 4.23, p > .2$.

Discussion. As predicted, children showed more intonationally-defined responses than did adults. Because, however, the intonational responses of two-year olds did not differ significantly from the baseline level of incorrect responses produced by children in the control condition, no

claims can be made concerning what the two-year olds were attending to. Three-year olds do, however, appear to judge similarity on the basis of stress pattern rather than phoneme sequence in some subset of the instances. That observation suggests that prosodic components of a speech sequence can, in some cases, be more prominent in the child's representation of a word than the phonemes of that sequence, and it is consistent with the claim that children may extract and store the stress pattern of a target utterance without a complete representation of the phonemes.

Although three-year olds and many two-year olds are past the initial phases of language learning, in which a tendency to attend to and extract intonationally-defined sequences should be most important, these observations are not irrelevant for understanding the earlier processes: A tendency important in the earliest stages of language acquisition would presumably not disappear immediately. Three-year olds should still show a greater tendency than adults to extract and store intonation as part of the representation of the form of a word. Although additional research, using a paradigm more suited to infants, will be required to provide more direct evidence for a very early role for the proposed tendencies, evidence for a tendency to attend to intonation in three-year olds, but not adults, does suggest that such a tendency may assist in early language acquisition.

It should be noted that other interpretations of these data are possible. Although the intonational responses of three-year olds, at least, were not simply errors, it is conceivable that children were confused by a test label which incorporated elements of both previously taught labels. Such uncertainty could have resulted in essentially chance responding. An anecdotal description of children's responses in the test situation may partially address that concern. If the test label confused children, then they should have responded more slowly and uncertainly on the test trial. Most children responded as quickly on the test trials as they did on the training trials. A few children did, however, respond by choosing both objects, gave responses such as "it's in the [researcher's] bag," or refused to choose any object. In those cases, the experimenter would say, "if one of these were an 'X,' which one would it be?" If the child did not then choose a single object, the response was recorded as uncodable and that child's score was, as described above, omitted from some analyses (this was true for a total of 5 responses from 4 two-year olds and for 3 responses from 2 three-year olds). Thus, although most children did not seem to be confused by the test label, some children did display evidence of uncertainty. This issue will be more fully addressed in a replication study through a control condition in which children hear an entirely novel label.

General Discussion

The research described here began with the notion that perceptual biases may assist in extracting or identifying words from speech. In the first part of the paper, evidence for the perceptual salience of stressed and final syllables was described: Young children less frequently omitted stressed and final syllables from their productions and, even where unstressed or nonfinal syllables were present, they were less accurate. Tendencies to attend to stressed and final syllables are not, however, sufficient to account for all first productions. A second study explored the possibility that an additional type of attention to stress may also assist children in identifying extactable elements of speech. Children may frequently attend to and extract not only the stressed syllables per se but also the stress *pattern*, or the overall intonation contour, of a sequence of speech. Where subjects were required to identify referents for words on the basis of either stress pattern or phoneme sequence, three-year olds were more likely than adults to make choices based on intonational similarity. Those observations suggest that children are more likely than are

adults to include intonation as an important part of the representation for a word. They are also at least consistent with the view that stress pattern, and possibly other aspects of intonation, may be extracted as part of the representation of a word and may, in some cases, serve to define a word for a child. Thus, it may be that the child is assisted in extracting and identifying first words not only by tendencies to attend to stressed and final syllables, but also by attention to the stress pattern or overall intonation contour of certain sequences.

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NONEGOCENTRIC USES OF "BIG" AND "LITTLE" BY PRESCHOOL CHILDREN

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Psychologists have assumed that children learn and use words that make sense to them. If so, early use of a word implies that a child has an early grasp of the underlying concept. The words "big" and "little" emerge very early -- between 1 and 2 years of age (Robb & Lord, 1981). Yet in spite of this, there is a longstanding impression in the literature that young children cannot understand the relative nature of "big" and "little." Early research suggested that relative judgments (choosing the smaller of two squares, for example) were more difficult to make than absolute judgments (e.g., Alberts & Ehrenfreund, 1951; Kuenne, 1946). Surprisingly, this impression has remained even though later research demonstrated that young children can in fact make relative judgments (e.g., Bryant, 1974; Sera & Smith, 1987). In a recent textbook on developmental psychology, for example, Shaffer (1985) writes, "... 2-3 year-olds do not truly understand the meaning of relational adjectives such as big or little. If a toddler can easily handle an object, he is likely to describe it as 'little'; but if the object is cumbersome or difficult to manipulate, it is 'big' ... Only later will children come to realize that big and little are relative terms, so that a motorbike might be described as 'big' when compared with a tricycle but 'little' when compared with a car" (p. 298).

Obviously the issue of what "big" and "little" mean to children remains unsettled. We suggest that one reason for the lack of agreement is that these terms are more complex than they seem. Dimensional adjectives are inherently relational and actually have no absolute meaning. In other words, whenever the term "big" or "little" is used to describe an object, the user must be taking into account some relationship between the object and a standard.

Adults use at least three different kinds of standards in judging whether something is big or little. The first standard is normative: an object is seen by itself, compared to some stored mental standard for objects of that kind, and judged relative to that standard. For example, a hat seen by itself is judged as big or little for a hat. A second use is perceptual: an object is seen with another object of the same type and its size is judged relative to that object. For example, two hats of different sizes are presented and one is judged as big or little relative to the other. A third use can be called functional: an object is examined in terms of how well it fulfills or could fulfill an intended function. For example, a hat can be judged as big or little for a doll, depending on how well it would cover the doll's head.

These three standards can conflict. For example, a hat can be big using a normative standard but little using a perceptual standard -- that is, big for a hat but little compared to the hat next to it. Or a hat can be little using a normative standard but big using a functional

standard -- that is, little for a rat but big for a doll that is extremely small. Adults are able to use context to decide which word to use. For example, if a rat is shown with a larger rat an adult could describe it as little, but if the same rat is shown with a tiny doll the adult could describe it as big. So correct use of the words "big" and "little" is not at all trivial: it means that an individual can use the terms in at least three different senses and can switch from one use to another depending on context.

The present studies examine whether young children can use "big" and "little" in these three ways, and whether they can use different standards in different contexts. Our basic findings are that children understand that these words describe relationships between objects, that they can use the terms "big" and "little" in all three ways described above, and that they can switch from one standard to another depending on context. We report two studies in the present paper. The first shows children's keen sensitivity to different relational standards; the second suggests that some standards are more difficult than others.

Experiment 1

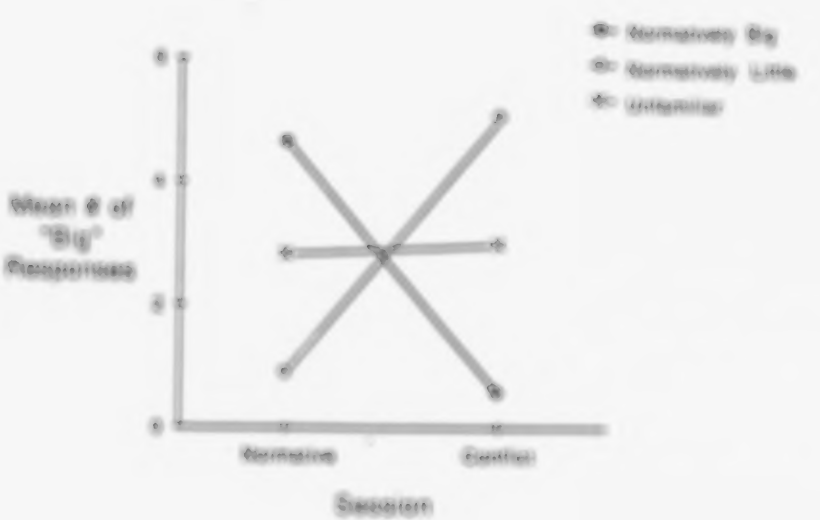
In the first study we focused on the two standards we hypothesized would be easiest for children to apply, normative and perceptual. There were two sessions, one examining children's ability to use normative standards and another designed to look at how children would perform when either a normative or a perceptual standard could be used. In the first session the experimenter brought out a variety of objects, one at a time, and asked the child whether each one was big or little. All of the objects were about the same size. However, some were normatively big, some were normatively little, and others were unfamiliar. For example, we included an egg that was nearly 6 inches long (big for an egg), a box of cereal that was 6 inches tall (comparable to the egg in its longest dimension but little for a box of cereal), and a bicycle reflector (about the same size as the egg and the box of cereal but relatively unfamiliar). We predicted that children would draw on their stored normal standards to label the familiar objects and therefore would call the egg "big" and the box of cereal "little." However, because the bicycle reflector was a relatively unfamiliar object, it was unlikely that children would have normative standards for it. Therefore we predicted that the bicycle reflector would not be consistently labeled as either "big" or "little." In all, children were tested on 18 items: six that were normatively big, six that were normatively little, and six that were unfamiliar.

In the second session, we asked children to judge the same objects they had already judged in the first session. This time, however, we arranged a conflict between two different standards in order to see whether the children could shift from one standard to another as context changed. Recall that in the first session items were presented one at a time, so that the only basis for judging size was relative to

a stored mental standard children had of other objects of that kind. In the second session children could still make a normative judgment, but we also made it possible for them to make a perceptual judgment by bringing out two objects of the same kind. For example, on one item we paired the 4-inch egg described earlier with an egg that was even bigger. Children were again asked whether the 4-inch egg was big or little. If children were still using a normative standard they would judge it as big; if they switched to using a perceptual standard they would judge it as little. Similarly, we paired the 4-inch box of cereal described earlier with an even smaller box of cereal. Children could judge the 4-inch box as either little (using a normative standard) or big (using a perceptual standard). Finally, we paired each unfamiliar object with another unfamiliar object of the same type. Half the time the new object was larger than the original object; half the time it was smaller. Because children probably do not have a strongly established sense of the normative size of unfamiliar objects, we expected they would make perceptual judgments with these items.

We conducted Experiment 1 with 12 2-year-olds (M = 2-9), 12 3-year-olds (M = 3-10), and 12 4-year-olds (M = 4-9). Each child was tested in both conditions: normative (seeing each object individually) and conflict (seeing each object paired with another of the same kind). For every trial, the child was asked whether the target object was big or little. For example, on the egg item, the experimenter said, "See this egg? Is it a big egg or a little egg?" The results are shown in figure 1.

Figure 1.



Subjects of all ages were able to use both kinds of relative standards, as seen by a significant object type x session interaction, $F(2,66) = 319.52$, $p < .0001$. In the normative condition children appropriately judged the normatively big objects as "big," the normatively little objects as "little," and the unfamiliar objects as "big" half the time and as "little" half the time. Since all of the objects were approximately the same absolute size, children had to use a stored mental standard to produce these correct results. In the conflict condition children switched to a perceptual standard. They judged the normatively big objects as "little" (because they were paired with objects of the same kind that were even bigger), the normatively little objects as "big" (because they were paired with objects of the same kind that were even smaller), and the unfamiliar objects as either big or little, depending on the size of the object next to them.

In other words, when an object is presented by itself, children judge it accurately according to a stored mental standard; when it is presented with another object of the same kind, they compare it to that object and make a perceptual judgment. There were no age differences; even the 2-1/2-year-olds performed very well. It is interesting that children switched so readily from a normative standard to a perceptual standard, depending on the context. The wording we used implied a normative standard in both conditions ("Is this a big egg or a little egg?"), yet children nearly always switched to a perceptual standard when they had a choice.

Experiment 2

In Experiment 2 we examined children's use of functional standards. This kind of judgment was first studied about 10 years ago by Susan Carey (reported in deVilliers & deVilliers, 1978, pp. 132-134). In Carey's study, young children were introduced to a set of dolls, with a table and tea set of the right size for the dolls. After a few minutes of playing "tea party," the experimenter told the children that the dolls needed a glass to drink from, and produced a shot glass which was of course small for a glass but enormous for its intended use. When 2- and 3-year-olds were asked whether the glass was "big" or "little" for the dolls, they incorrectly said it was little. This was an incorrect response because the glass was intended for the dolls and was much too big for them to drink out of. Four-year-olds, on the other hand, answered correctly that the shot glass was big for the dolls.

It seemed from this study that young children have difficulty with functional judgments. Apparently they judged the glass from their own perspective -- little for themselves -- rather than from the doll's perspective. The task we developed allowed us to study children's use of functional standards in more depth.

On each item of our task, children judged the size of an object relative to a doll. In order to assess both "little" and "big," we used two dolls, one that was large (15" or long) and one that was small

(12 cm long). Each doll was shown with eight items, four of which were clothes and four of which were tools. The clothes included items such as a mitten, shoe, and shirt; the tools included items such as a cup, toothbrush, and scissors. We included both clothes and tools because children might have more experience judging clothes for a doll.

Children were shown each object with the appropriate doll and were asked whether the object was big or little for the doll. Items shown with the big doll were too little; items shown with the little doll were too big. For example, the big doll was shown with a shoe that was normatively large, but too small for the doll to put on her foot. Similarly, the little doll was shown with a hat that was normatively little, but completely covered her head. In general, items were chosen so that the misfit was fairly obvious.

To summarize, in Experiment 2 we set up another conflict situation, but in this case the conflict was between normative and functional standards, instead of between normative and perceptual standards. The objects used with the little doll were normatively little but big for the doll. The objects used with the big doll were normatively big but little for the doll.

We also included a condition in which children judged the size of each object presented by itself. This condition, which we call the Normative condition, was included as a control, to ensure that children knew the typical sizes of the objects we were using. In the Normative condition we used the same items as in the Doll condition, but children never saw the dolls. They were simply shown each object, one at a time, and were asked whether it was big or little.

If children are sensitive to both normative and functional standards they should label the objects differently in the two conditions. They should label the little objects as "little" in the Normative condition, but "big" in the Doll condition. Similarly, children should label the big objects as "big" in the Normative condition, but "little" in the Doll condition.

72 children participated in the study, 24 3-year-olds (M = 3-6), 24 4-year-olds (M = 4-6), and 24 5-year-olds (M = 5-6). At each age, there were 12 children in the Normative condition and 12 in the Doll condition.

As shown in Figure 2, there were three main findings. First, children in all three age groups in both conditions performed above chance overall, $p < .05$. So children as young as age 3 are capable of making nonegocentric functional judgments. In fact, it is remarkable how well children accepted their misfits. They were extremely interested in how the objects themselves. For example, they would try to put the mitten on their fingers or would try the watch on their own wrist.

The second finding was that performance on the Doll task improved with age, as shown by an age x condition interaction, $F(2,66) = 7.15$, $p < .002$. This suggests that even though 3-year-olds can interpret "big" and "little" in nonegocentric ways, they sometimes find it difficult. These conclusions were supported by the spontaneous comments of some of the children. One 3-year-old, who was shown the scissors for the little

Figure 2.

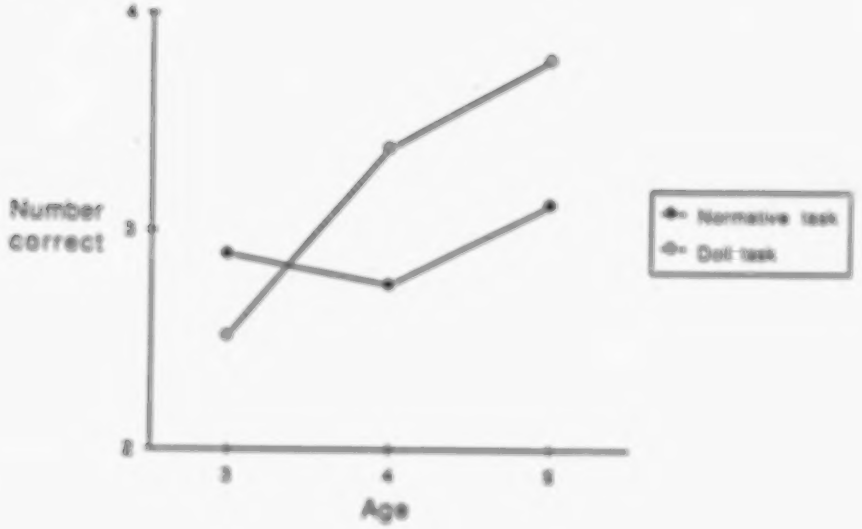
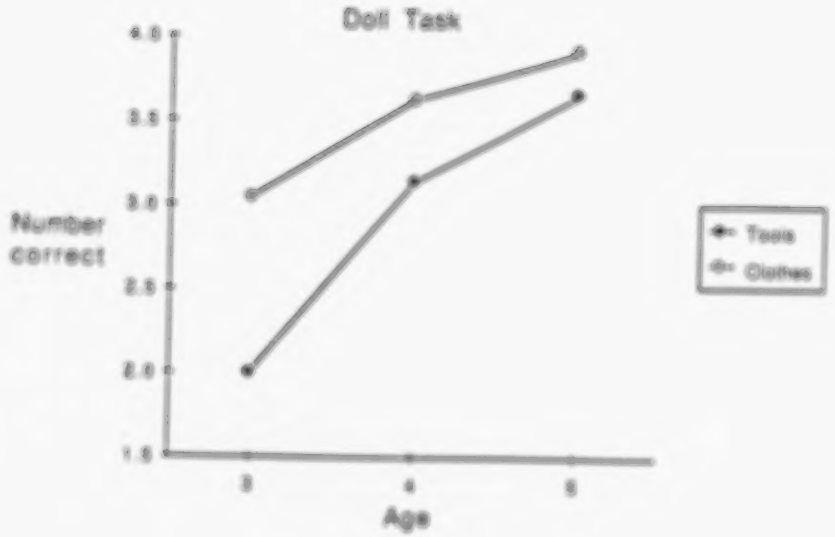


Figure 3.



doll, said, "[it's] big, because you're supposed to be able to use your fingers and she has small fingers." Another child, age 4, when shown the cup with the big doll said, "it's little. It's good for people to drink out of but not for her [the big doll]." On rare occasions even 3-year-olds sometimes spontaneously justified their answers. For example, one child when shown the shoe with the big doll said, "[it's] little, because the doll has big feet." Another 3-year-old, when shown a comb with the big doll, correctly pointed out, "it's little and it's big for me."

In contrast, there were no changes with age on the Normative task. This is consistent with the finding in Experiment 1 that even young children could readily make normative judgments.

The third finding from this study is shown in Figure 3. Children at all ages judged clothing for the doll more accurately than tools for the doll, as seen in an object type x condition interaction, $F(1,66) = 6.46$, $p < .02$. The youngest children especially had difficulty and performed at chance when judging tools relative to the dolls. (This replicates what Carey had found: young children could not judge a normatively little cup relative to a little doll.)

It is not clear why children found it easier to judge clothes than tools. As mentioned before, one possibility is that children have more experience talking about the size of clothing and trying clothes on dolls than they do talking about the size of tools. Another possibility is that children found it difficult to judge tools because they had to make spatial inferences on such items. The clothes were actually tried on the doll and children could observe that they did not fit. In contrast, although the tools were placed in the doll's hand, children still had to figure out which comparison to make. When judging whether the pair of scissors was big or little for the doll, for example, they had to compare the size of the scissors with the size of the doll's hand and then imagine what it would be like if the doll were actually using the scissors.

To summarize, there were two main findings from these experiments. First, by age 2-1/2 children understand that "big" and "little" are relative terms. They can use either a normative or a perceptual standard as a basis for judging an object to be "big" or "little," and can switch between these two standards depending on context. Second, by age 3 children can judge the size of an object in relation to its intended use, as long as that relationship can be observed. Problems do seem to arise when the relationship must be inferred.

From our view, the most impressive finding concerns the complexity of these words and the skill with which they are used. Children are fully aware of the relational meanings of "big" and "little," they can judge an object as "big" or "little" according to three different standards, and they are sensitive to context in deciding which standard to use. Compared to these achievements, a few minor difficulties in making functional judgments hardly seem like a problem at all.

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Texts within Texts: A Developmental Study of Children's Play Narratives

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Research on children's narratives has shown that, by the time they reach the age of five, children have the ability to produce a number of kinds of narrative discourse, or narrative genres. Even very young children can produce scripts, or generalized representations of routinely experienced events such as birthday parties, trips to the grocery store, and lunchtime activities (Nelson and Gruendel, 1981; Nelson, 1986). In addition, children as young as two years of age can produce what Heath (1986) terms recounts, or narrative productions scaffolded by a co-present adult (Eisenberg, 1985; Stoel-Gammon and Scliar Cabral, 1977). Young children can also produce accounts, or narratives which the child initiates and relates independently of a co-present adult (Eisenberg, 1985; Heath, 1986; Miller and Sperry, 1987; Peterson and McCabe, 1983; Umiker-Sebeok, 1978). And finally, children as young as two or three years of age can engage in fictional storytelling as they recreate real-life events and social roles in a make-believe context (Applebee, 1978; Rubin and Wolf, 1979; Scarlett and Wolf, 1979; Sutton-Smith, Botvin, and Mahoney, 1976).

This very brief summary of the literature on children's narratives suggests that an important part of language development is, first, the ability to represent events and actions through narrative discourse and, second, the ability to produce a range of narrative genres appropriate for specific occasions of language use. The ability to produce both fictional narratives about the exploits of imaginary animals and factual accounts of events from earlier in the day enables children to participate more fully in home and school settings. Yet, the present research on children's play narratives suggests that narrative development is even more complex: that one may find even within one narrative genre, or text, a number of strands of discourse representing the different perspectives on events which speakers may take.

The present study is a linguistic analysis of the play narratives produced by eight children observed longitudinally between the ages of two and seven. In this study, part of a larger research study of symbolic development conducted by Dennis Wolf and Howard Gardner of Project Zero, children were visited in their homes and were videotaped as they played with small toy figures: animals, monsters, giants, and people living in "strange forests". Children were given a story opener and were then encouraged to create fictional stories as they manipulated the replica characters in their play. The resulting play narratives were transcribed, accompanied by notes about children's voice quality (pitch, intonation) as well as gestural movements and eye gaze.

Voices in early play narratives

In our examinations of these play narratives, we have found that, although one can consider these texts representative of a certain genre -- that is, fantasy play narratives -- one also finds that the narratives

from a very early age contain two and sometimes three strands of text. In the example shown below, Heather at age 2;7 is narrating a story about a lion attacking a family of bears. In her narrative, Heather moves back and forth between two lines of text, or voices: a narrative voice, in which she recounts the actions of the animals, and a dialogue voice, in which characters give commands and provide "sound effects". Her story illustrates that the narratives of even very young children may be multivocal as opposed to being one singular text.

Example 1: Heather (2;7)

narrative	dialogue
Now, he [the big lion] is going far, far away. And they going in their house. And then the babies gonna cry and then the daddies gonna take care of them .... Then the baby bear gets to his daddy. Then they don't get scared and the kids come too. They don't want to be eaten up.	
	(lion): Roar, roar, roar. Go back in your house.
He go back to that house.	

Given this orientation towards the replica play narrative data, our analysis of the data proceeded as follows. The transcribed narrative texts were first segmented into clause units, with the assumption that any clause containing a predicate construction (verb phrase) constituted a separate unit of analysis (Berman and Slobin, 1984). Individual clauses were then designated as being representative of one of three voices: a narrative voice, in which children recount "what happened" or "what is happening" in the story; a dialogue voice, in which children encode the conversations and internal thoughts of characters; and a stage managing voice, in which children negotiate with the listener about the roles of characters, props, and settings in the play. In the interest of time, our focus in this paper will be on children's use of two of these voices, or lines of text -- dialogue and narrative -- since these constitute the majority of the narrative clauses in our data set (see Wolf and Hicks, in press, for an analysis of three strands of text in children's narratives).

The particular function (narrative or dialogue) performed by individual clauses in the narratives was decided on the basis of pragmatic cues as well as linguistic structure. Children's eye gaze and gestural movements were crucial in determining whether a particular clause was uttered from the perspective of an omniscient narrator or of one of the characters in the play. In addition, the use of pronominal forms and of various clausal types was an important determinant of the function of clauses. If, for example, a child in a neutral voice tone and with her gaze directed towards her replica characters announced, "he's going into the house", this clause was considered representative of the narrative voice. If, however, a child in a high (or low) pitched voice and while manipulating one of the characters uttered the imperative, "get back in the house!", this clause was considered dialogic in nature.

After segmentation of individual clauses in the narratives into separate voices, clauses were coded in terms of children's use of three linguistic systems representing widely different dimensions of narrative structure: the use of various pronominal forms -- first, second, and third person forms as well as deictic forms; the use of clausal types-- declarative, interrogative, and imperative; and finally the use of various temporal forms (broadly defined) -- verb inflections, sequencers, and verb semantic types. The proportional use of individual linguistic forms, taken from either the total number of clauses of one functional type (narrative or dialogue) or, in the case of pronominals, from the total number of pronominal forms within clauses of one functional type, was then examined to assess possible developmental changes in the data.

Developmental changes in children's play narratives

Given that children's use of pronominal forms and clausal types was in part used to determine the particular function (dialogue or narrative) of clauses, the focus in the analysis of the data will be on developmental changes in the use of temporal forms, including semantic verb types. Very briefly, however, there were no developmental changes found in the use of either pronominal forms or clausal types. Children between the ages of three and six used third person pronominal forms in nearly all narrative clauses and used a mixture of first, second, and third person pronominals in dialogue clauses. In addition, children at all points in time used declarative clausal forms in narrative clauses but used a mixture of declaratives, imperatives, and questions in dialogue segments.

There were, however, some interesting developmental changes in terms of children's use of the temporal system. First, as was shown in the earlier example of Heather's narrative at age 2;7, very young children tend to use the present tense, along with gonna and the English progressive form, to describe ongoing events in their replica play narratives. Between the ages of three and six, however, one finds a dramatic increase in the use of past tense forms to encode events in the narrative voice, whereas in the dialogue voice one finds a steady use of present tense forms across age levels. In Figures 1 and 2 shown below, which illustrate this developmental change, data from narratives are collapsed into four age levels. All data from narratives between the ages of 2;7 and 3;6, for example, have been collapsed into the age level of three years.

-- insert Figures 1 and 2 here --

In addition to this change over time, there were changes both in the use of semantic verb types and in the use of temporal sequencers. Verbs were coded as being one of four semantic types: event verbs, encoding goal-directed actions such as take, go, scare; process verbs, encoding non-punctative actions such as swim, play, and jump; internal state verbs, encoding character states such as feel, think, and be-happy; and finally relational verbs, encoding physical states such as be-in-the-pond, stay, and be-a-giant. In Figures 3 and 4 below, the percentage use of three of these semantic verb types -- event, internal state, and relational -- is shown for four age levels and within the two strands of text, narrative and dialogue. One can see from these figures that there is an increase over time in the use of event verb types in the narrative strands, whereas

in the dialogue strands there is an increase over time in the use of internal state and relational verbs.

-- insert Figures 3 and 4 here --

Finally, our analyses of the data revealed developmental changes in children's use of additive (and) and of sequential (then, and-then) connectives. Within the narrative strands of text, there was an increase over time in the use of both additive and sequential connectives, whereas within dialogue strands of text, there was almost no use of these kinds of linguistic forms. Figures 5 and 6 below illustrate this pattern in the use of sequencers, and Figure 5 illustrates in addition a slight decrease between the ages of five and six in the use of additive forms. This decrease may be due to some changes, to be discussed in a section below, in the ways in which children encode events within narrative strands.

-- insert Figures 5 and 6 here --

This differential use of the temporal system within strands of text may best be illustrated through case studies of two of the eight children in our study. In the example below, Heather, the same child whose narrative at age 2;7 was examined earlier, creates a story about a family in the "strange forest". In contrast to her narrative at 2;7, however, her play narrative at age 3;5 contains past tense forms, sequencers, and a large number of event type verbs within narrative strands. In the dialogue strands, Heather uses the present tense as she did at age 2;7, but at age 3;5 she also focuses more heavily on the physical and internal states of characters.

Example 2: Heather (3;5)

narrative	dialogue
Once upon a time the baby and the mommy and the daddy, they walked through the forest to find a house and said ...	
	There's a porch.
Then they found a porch! She found a porch, and then he found a porch, and then they walked through the forest.	
	(baby): Where's my mommy and daddy?
And then the mommy goes ...	
	(mommy): Hi (baby): Hi, Mommy. I want to go for a walk.

In the additional examples shown below, Jonathan at age 3;6 uses past tense forms to encode events in narrative strands and uses present tense forms and gonna to encode dialogue strands. At age 4;11, one also finds that the character speech within the dialogue strands contains references to characters' thoughts and feelings.

Example 3: Jonathan (3;6)

narrative	dialogue
The monster came struggling. Walked down to the tippy, wippy little house.	(monster): I'm gonna knock your ... knock, knock, knock, who's there?

Example 4: Jonathan (4;11)

narrative	dialogue
This was the land of funny men. And then the guy [an animal] came. And he said ...	(animal): What's the matter? (giant): I got lost. Well, what's this land called? (animal): Funny land. What do you think it's called?

The endpoint of the study: the narratives of five and six year olds

After age four, and generally between the ages of five and six, one finds evidence of an increasing versatility in how children move between narrative and dialogue strands of text. Children by age five begin to demonstrate that a variety of functions can be performed within these two voices. Narrative strands in general represent "what happened" in the story, but one can also within narrative strands make comments on characters' emotional and physical states. Dialogue strands in general represent online conversation between characters, but the omniscient narrator can also use dialogue to both recall and project events. The examples shown below from Heather at age 4;11 and Jonathan at age 5;11 illustrate this additional kind of developmental phenomenon.

Example 5: Heather (4;11)

narrative	dialogue
	(giraffe): I'll eat some grass. Yum, yum. Let me eat some leaves from the earthquake last night. Did you know that there was an earthquake last night? (duck): Oh, no. (animals to giant): What have you done? (giant): [I] took your bath and

I made a little pond for you. I gave you a nice present to the little animals. They lived in the forest once long ago. They came this year ....

Example 6: Jonathan (5;11)

narrative	dialogue
For days and days the giant made the pond his bath. The animals were thirsty and they usually had fights with the little wierdos. And then ... one day the giant came back.	
	(giant): Ahhh, trees.
They're apple trees.	
	(giant): I shall use these apples for me to eat. I shall take all of them.
So he ate apples and was happy	

In Heather's narrative age age 4;11, the character of the giant recalls some previous events in the story, so that his utterances sound like those which might have been previously found only within narrative strands of text. And yet, Heather by age five has the linguistic skills enabling her to represent past events in the voice of one of the replica play characters. Similarly, in Jonathan's narrative at age 5;11, the character of the giant projects events which will occur in the story ("I shall use these apples for me to eat"), and the narrator of the story both recasts the giant's speech ("so he ate apples") and expresses the emotional status of the giant ("and was happy"). These examples point out the complexity of children's play narratives by age six: not only are children's narratives multivocal, but separate voices may also perform a wide variety of functions in the narrative.

Conclusion: Intertextuality in children's play narratives

The findings presented in this paper suggest certain developmental changes in the way in which children between the ages of three and six mark different strands of text in their narratives. The data from the study suggest that the play narratives of even very young children may be multivocal, consisting of an interplay of voices representing the different perspectives which the speaker assumes with respect to events. This research supports the hypothesis that narrative development involves not only the acquisition of various genres, such as scripts, personal accounts, and stories; it also involves the mastery of skill in manipulating strands of text within genres. Perhaps it is then useful to look at children's narratives not only in terms of the development of skill in representing events and actions but also in terms of children's utilization of linguistic systems to represent texts within texts.

Figure 1.
Percentage Use of Verb Inflections in Narrative Strands

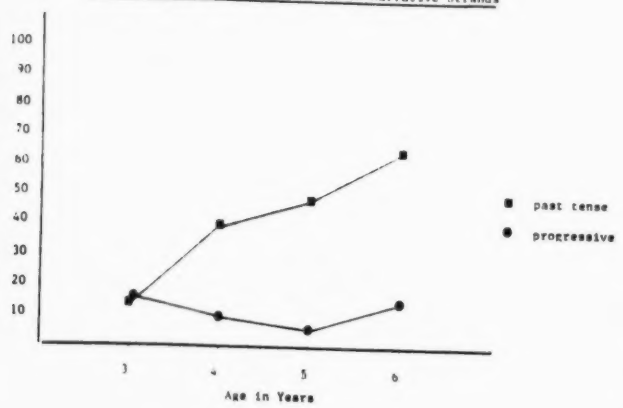


Figure 2.
Percentage Use of Verb Inflections in Dialogue Strands

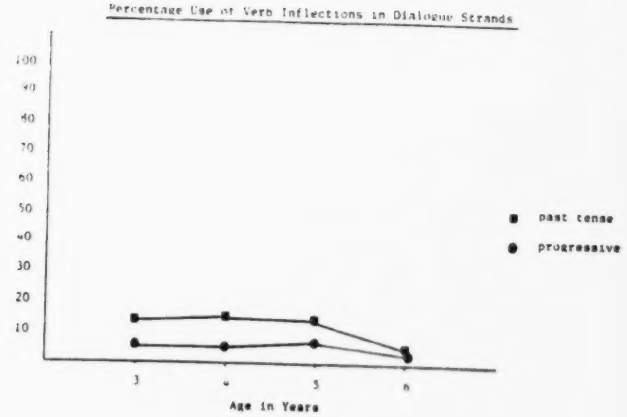


Figure 3.
Percentage Use of Verb Types in Narrative Strands

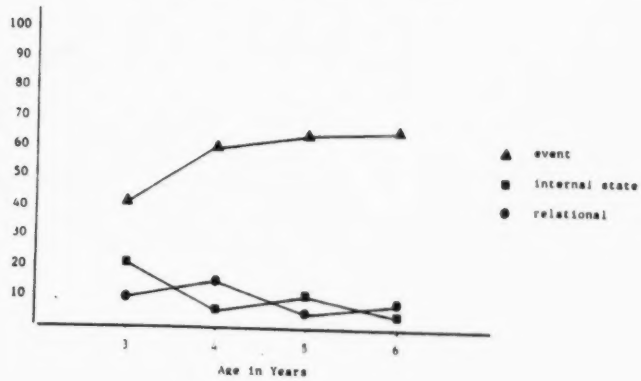


Figure 4.
Percentage Use of Verb Types in Dialogue Strands

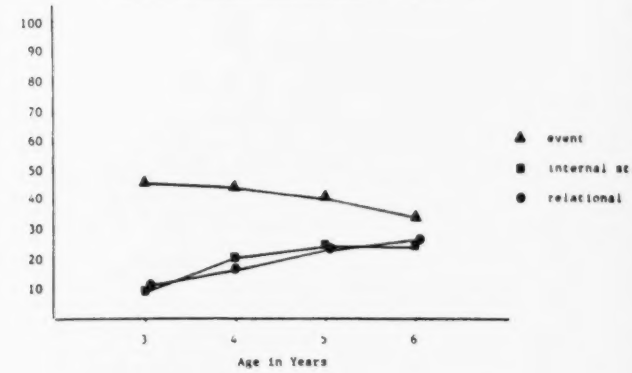


Figure 5.
Percentage Use of Additive and Sequential Connectives in Narrative Strands

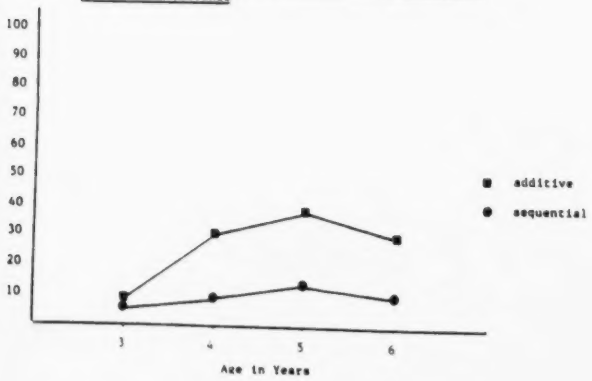
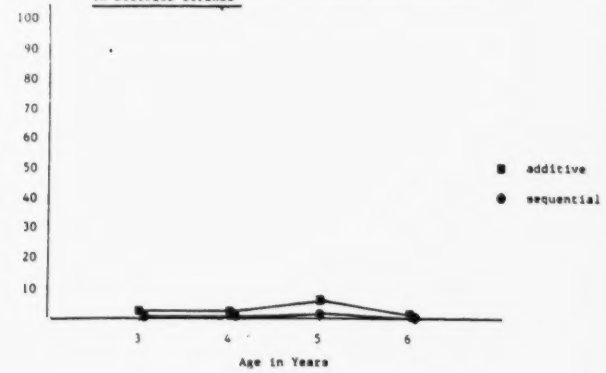


Figure 6.
Percentage Use of Additive and Sequential Connectives in Dialogue Strands



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ANOTHER LOOK AT CHILDREN'S INTERPRETATION OF COMPLEMENT-TO CONSTRUCTIONS

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Recent work in the area of child language in light of developments in syntactic theory has raised a number of issues affecting in its own right the way we look at the growth of language. We are asking ourselves questions concerning how one should view the process(es) of language development. We must now consider how some view influences what we consider as relevant phenomena for study to begin with, reasonable analyses of the phenomena, and predictions about the emergence of other possibly related phenomena. The underlying issues themselves, nonetheless, are not new. In large measure the questions they elicit and the issues they focus on revolve around a cluster of questions central to linguistic theory throughout its recorded history.

The cluster has to do essentially with the notions *same/different*, with contrast, identity, and variation. Current basic theory driven research has as a central goal the determination of how different any one language can be from any other language, given the biophysical uniformity across human beings that is characteristic of the brain what we typically take to be the repository for linguistic knowledge. This goal automatically becomes a question about the development of language in children: how is it possible for a child, with his or her neurophysiological makeup, to develop any one of the human languages or dialects that exist? A major part of the answer to this question will come from the specification of Universal Grammar, which a number of researchers take to be the characterization of the biological endowment providing for the internalization of linguistic knowledge, the initial state. But there is a corollary set of questions that must be addressed within this framework. Children's language differs from adult language in easily observable ways. The first question is just how different will (in fact, can) an analysis of a child's linguistic system, presumably underlying these observable differences at some given time, be from the adult system that defines the endpoint of language development? We ask further what the source of the difference must be, what not only permits it, but makes the difference inevitable. Thirdly, we ask how the difference is resolved. As we know from experience, unlike the differences between dialects and languages, the differences between child language and adult language typically resolve. Interesting with this corollary set are the questions generated by our views of the process of language acquisition. How does the picture of an instantaneous model of language acquisition correspond to the presumption of intermediate systems in the course of language development? How does the development of linguistic knowledge interact with the development of knowledge in other domains: how do these domains affect one another? And finally, how does our view of a model of linguistic structure as modules affect the way we perceive (and ultimately, understand) the course of language development?

The discussion in this paper will address these questions in the context of children's apparently different interpretations of *wh*-movement structures, such as complements to *easy to please* (Chomsky's own) and a subsequent study carried out by Richard Cromer. The discussion assumes the theoretical framework outlined in Chomsky (1981) and (1986), exercising a number of the descriptive mechanisms I provide for in my monograph, *The Acquisition of Syntax in Children from 5 to 15* detailing work completed just about twenty years ago. Carol Chomsky did address issues relating to differences for from a distinct point of view. Focusing on notions of complexity, she sought to distinguish constructions, one from the other, on the basis of the extent to which their varying levels of complexity would be reflected in their appearance in children's language. One of the sets of different constructions Chomsky noted appear as (1) here.

1. a. *John is eager to please.*
   b. *John is easy to please.*

She argued that (b) of the two well-known, now classical pair of sentences represents a higher level of complexity. She attributed this characterization to the claim that the grammatical relations in (b) are not represented directly in any way given that its structure is identical to the structure of its counterpart in (a)

with quite different (but what was claimed to be the canonical) grammatical relations. The schemata in (2) sketch the relevant issue here, which involve the interpretation of the empty categories marked as e. The subscripts indicate indexing, which we assume is part of the specification of any NP.

2. a. [John$_i$ is eager [e$_i$ to please e$_k$]]
   b. [John$_i$ is easy [e$_k$ to please e$_i$]]

In the context of these assumptions and claims, Chomsky predicted that children would first interpret sentences such as (3b) as if they were structured like (2a), rather than (2b). Using the sentences in (3), and a stuffed-doll Charly Cathy doll, she carried out one of the earlier psycholinguistic experiments designed, as Cowper has recently characterized the role of experimentation, "to measure the deductive capacity of linguistic theory." (Cowper 1992).

3. a. The doll is easy to see.
   b. Is the doll easy or hard to see?

Indeed, Chomsky found that boys as old as eight years and five months, and girls as old as six years, six months seemed to interpret sentences in (3) as though the missing subject of see were the NP the doll, answering the question in (3b) with 'hard to see.' Questioned further, with 'Would you make her easy to see,' these children proceeded to remove the blindfold.

On the basis of Chomsky's work, Richard Cromer tested forty-one children between the ages of five and seven in a related experimental situation. He first categorized the children in terms of their performance on the Peabody Picture Vocabulary test (PPVT), using what he refers to as "mental age," computed on the basis of a child's relative success with this vocabulary test. Then, using a pair of hand puppets, a duck and a wolf, he tested children's understanding of complements to three categories of adjectives, exemplified by the sentences in (4).

4. a. The duck/wolf is anxious to bite.
   b. The duck/wolf is fun to bite.
   c. The duck/wolf is nice to bite.

(4a) and (4b) of course, reflect the distinction in (2a) and (2b) respectively. (4c) is an ambiguous sentence. It could have an interpretation consistent with either (2a) or (2b). In addition to the difference in the adjectives that he tested, Cromer introduced a slightly different methodology as well. Providing children to be study with the pair of puppets (a duck and a wolf), the investigator asked these children to show (act out) a sentence just uttered. Beginning with prompts such as "Show me the duck bites the wolf," the experiment moved through sentences such as those in (4). Cromer also introduced two nonsense adjectives, *ziggy* and *lerrup*. Presenting them as (5) indicates, an experimenter would then proceed to ask a child to depict an interpretation of the sentence "The wolf is $\begin{bmatrix} \text{ziggy} \\ \text{lerrup} \end{bmatrix}$ to bite."

5. a. Say: Someone gave this dog a bone. So he's feeling very ziggy. He's feeling very ziggy.
   b. This cat climbed up and picked a rose. And he found that chewing the rose was lerrup. Chewing the rose was lerrup.

Supporting Chomsky's findings, Cromer's results reflected that children in the experiment whose "mental age" on the PPVT was less than six years had the subject of the predicate adjective in what we are analyzing as the matrix clause carry out the action in the adjectives (embedded) infinitive to all cases.

Some interesting variation that Cromer reported merits our attention here, as it will figure in our subsequent discussion. Rather than falling into precisely two groups, a constant subject analysis group vs group Cromer referred to as "primitive role users") and a group whose responses consistently reflect what

[illegible] the "passive"] the children in Cromer's experiment also formed a third group, which he named "the intermediates." Children in this third group "gave mixed answers--sometimes [using] the named animal and sometimes [using] the other--none of these being wrong" (Cromer 1970, p. 401). In addition, sixteen children were retested a few days after the first, twelve gave answers that were different from the ones they had given the previous day. Two children in this "inconsistent" group, (mental ages 4.11 and 5.8, respectively), had on the previous day given adult type answers. One on this retest changed to a mixed set of answers (some adult type, some subject only analysis), and the other "reverted to the primitive rule." (Cromer, *op. cit.* p. 406)

The nonsense word results are also interesting. Children falling into the first group--the subject analysis group--invariably used the subject of the matrix predicate adjective as the actor for the infinitive. The intermediate group, "while predominantly [using the matrix subject], also includes some cases in which children assigned deep subject status to 'the other' in one or both instances, *but incorrectly*. Passives, on the other hand, assigned deep subject status to the surface subject in one case and to the 'other' in the second case, and did so correctly" (ibid., p. 403, italics mine, SMK).

These results, together with Chomsky's provide us with the questions we need to ask about difference. What precisely is the nature of the knowledge (the grammatical system) Cromer's (and Chomsky's) primitive rule users have internalized? Does the experimental paradigm reinforce its use? What motivates the inconsistent group to be so? What is the relationship of the inconsistent and intermediate groups to an evolutionary model of acquisition? Corollary to this question is the question of how children come to be "passive". What is the nature of the complexity that Chomsky imputes to structures of the *John is easy to please* type? What insights about the children's underlying systems can the results of the nonsense word subset in Cromer's work help us develop?

It is not surprising to find that the questions themselves interact; proposals for answers to one affect subsequent answers and even change the questions. To begin with, it is quite likely that even the primitive rule users are not incapable of assigning an adult structural description (whatever that turns out to be) to the so-called rough-movement type constructions. The claim is that underlying these children's "incorrect" responses in both Cromer's and Chomsky's experiments is not an overriding rule of subject control. Rather, the responses reflect an interaction of a set of systems. One underlies a causative interpretation, affected by the children's available grammatical system and the conditions of the experiment. The second is the learning of the vocabulary itself--more precisely, children's learning of the capacity of the predicate adjectives to assign semantic (theta) roles to their subjects. Thirdly, we have the issue of the children's knowledge of the rough-movement type structures themselves.

We begin with the causative issue. It proposes that many of the children's "incorrect" responses for sentences such as (3b) and (4b) in the two experiments are the consequence of their construction of causatives for an *eat*-like , with themselves as the agents of these causative transitives. In other words, the structures underlying the children's interpretations of the sentences in (6a) and (6b) respectively are (7a) and (7b).

6. a. The doll is easy to see.
   b. The dark wolf is fun to bite.

7. a. the doll$_i$ is easy [PRO$_{arb}$ to cause [PRO$_i$ to see]]
   b. the dark$_j$ is fun [PRO$_{arb}$ to cause [PRO$_j$ to bite up]]

A number of studies (Bowerman 1982a, b, c, 1983, 1987; Lord 1975, Bates and Wexler 1987) have noted the productivity of the causative construction in children's language. Examples such as those in (8)-(10) are abundant in the literature. (The examples in (8) and (9) are from Bowerman, those in (10) are from Lord)

8. a. I don't want any more grapes; they just caught me (2.6 cited in Bowerman from Braine 1971)
   b. Don't giggle me. (3.0)

c. I want to **comfortable** you (5:9)

9. a. He **tippitoed** to the graveyard and **unburied** her. (5:1)
   b. How do you **unsqueeze** it? (3:11)
   c. Mother: (grabbing child in a game) I have to capture you.
      Child: **Uncapture** me! (3:10)

10. a. We have two kinds of corn: popcorn, and corn. Popcorn, it crunches. And corn doesn't crunch; it **eats** (3:3)
    b. You can **drink** me the milk. (3:8)
    c. I am trying to **guess** Aunt Ruth what I have (4:8)

In (9), intransitive verbs and adjectives are shown to participate in causative transitive constructions. In (8) we see what have been referred to as novel un-verbs. I have argued (Klein 1984) that children interpret predicates with **un-X-ed** as passive participles, and from these, deduce the corresponding active verbs that appear as the novel forms. What motivates them to move in this deductive direction is consistent with Lebeaux's claim that children are sensitive to a principle along the lines of (11).

11. a. affected [NPs] are internal arguments of verbs
    b. $NP_i \quad [_{VP} \; V \; t_i \;]$
       [affected]

In his analysis, such a principle functions as a trigger for the understanding of passives, by motivating the presumption of a trace internal to the VP, on the basis of the affectedness of the subject, giving structures such as (10b). Such a principle would operate as well in intransitive structures where the subject is interpreted by the child as affected.[1] Interestingly, in transitive verb constructions, if the verb participates in intransitive strings as well (as do verbs such as **eat**, **drink**, and **guess**, for example), we would expect children to deduce the causatives that we see in (10). Imputing to children the interpretation of the subject in such constructions as affected, we can see the source the sentences in (10). **Eat** allows both the intransitive in (10a) and the causative **I can't eat her**.[2] A structure such as (12) corresponding to their causative meanings underlies both (10b) and (10c).

12. $NP_1 \; [_{VP} \; [_V \left\{ \begin{matrix} guess \\ drink \end{matrix} \right\} \; NP_2 \;] \; NP_3 \;]$

The requirements of case assignment to the NP *the milk* are satisfied if the string **drink** /**guess** NP_2 is analyzed as V, which can then then license the assignment of case to the subsequent NP. In (13) the structure this framework would provide for **I drink the milk** appears.

13. I $[_{VP} [_V$ drink t $]$ the milk $]$

Faced with the experimental situations we have described, young children are very likely to construct analyses of sentences such as *the doll is easy to see or the wolf is fun to bite* with structures paralleling those underlying utterances (10b) and (10c). Understanding that the verb *bite* appears in both sentences such as **the wolf bites** and **the wolf bites the duck**, and wearing two hand puppets that he or she has been instructed to manipulate, the child in Cromer's study is invited to interpret him or herself as an agent of which the utterance given (*the wolf/duck is fun to bite*) is to be predicated.[3] It is equally inviting for young children in Chomsky's study to respond with this interpretation. In order to answer "correctly" there, a child not only must have a grammar that does not so readily permit the causative reading , but s/he must also be able to deal with conversational openings in testing situations. The question, "Is this doll easy to see or hard to see?" is incongruous as a sincere question in the context of a blindfolded doll. Any readers who have seen the film by deVilliers and deVilliers, "Out of the Mouths of Babes" will have cried the

responses of the older children in the illustrated replication of the Chomsky study; they smile knowingly at the examiner, answering "neither," and are then willing to discuss the "silliness" of the question. Moreover, Hughes and Grieve (1980) (cited in Menten 1987) found that children would try to give reasonable answers to anomalous questions, answers that were related to the experimental context or informed by the child's own experience. In these contexts, a young child who assumes that the question is a sincere one is very likely to resort to the causative interpretation.

The role of lexical learning interacts here as well. I argue that the causative construction during a period in child language is as productive as it is because a principle such as (11) interacts with the syntax at this time, licensing the movement of an internal argument in a [V NP] string to the external argument position, the position of grammatical subject. As the central role of a principle like (11) is supplanted by the use of passive morphology to deduce a VP internal trace, children will not have (11) to operate on non-morphologically marked intransitives.[6] In the absence of positive evidence supporting the presence of a syntactic trace (and thus movement) children will be forced to wait for positive evidence that causatives may be formed for *any* given verb, or that some intransitive verb does indeed participate in ergative constructions. Syntactic causatives will be unavailable for these children as an interpretive retreat in experimental situations such as the ones we have been examining.

We should, then, in slightly older children (or children who are lexically more sophisticated, a connection that Cromer's use of PPVT performance as an alternative metric to chronological age affords us) be able to look at the development of the *tough movement* structures without the veil of the causative issue obscuring our view of children's knowledge at an earlier stage. The presence of the intermediate group in Cromer's study allows us two inferences in the context of the framework we are using. First, we may infer that children do control the structure underlying these constructions, and second, we may infer that they must learn the syntactic context in which it occurs for each adjective.

Tough movement structures in Chomsky (1981) are given two possible analyses. In (14), one appears. Here, the structure of a sentence such as the wolf is hard to bite would look like (14a), and the structure of it is hard to bite the wolf, like (14b)

14. a. [e_P is $[_{A'P}$ $[_A$ hard $[_{CP}$ PRO_{arb} to bite]] $[_{NP}$ the wolf]]]

    b. [e_P is $[_{A'P}$ hard $[_{CP}$ PRO_{arb} to $[_{VP}$ $[_V$ bite $[_{NP}$ the wolf]]]]]

In (14a), the string hard to bite is itself analyzed as an internally complex lexical unit, an adjective, which, failing to assign case to its NP complement forces the movement of this complement to the empty e_P position. In (14b), the adjective hard takes a complement, out of which the binding conditions prevent, and the structural adjacency of the verb and NP object, obviate, movement. In the absence of movement, independent conditions provide for the presence of the non-referential it in the empty e_P position. In other words, some adjectives will have dual lexical entries, including one in which a string with internal clause structure is stored as a lexical unit. The account in (15) grows out of work in Chomsky (1977)

15. a. [the wolf is $[_{A'P}$ hard $[_{CP}$ e [IP PRO_{arb} to bite O]]]

Like the account in (14), the adjective here has two lexical entries. It is the one that would correspond to (15), the predicate does assign a theta role to the subject position, and takes a complement in which there is movement of a null operator, O, to the empty complementizer position, e. The entire clause (CP) is construed with the NP the wolf in a manner parallel to the construal of relative clauses with the NP head they modify. This yields a construction much like the construction proposed for infinitival structures, such as toys are to play with. The other lexical entry for the adjective would yield a structure virtually equivalent to (14b). These are quite complex analyses, and both pose problems for the theories in which they exist (cf Chomsky 1981 and Lasnik and Uriagereka 1988). Nonetheless, the complexity for children

does not seem to be a function of the intricacy of either account. Children much younger (chronologically, and given the gap, presumably lexically as well) regularly produce strings like toys are for to play with.[5] Such structures too have been analyzed as instances of operator movement (Chomsky 1977), so their presence suggests the ability of children to analyze such constructions. The presence of simple WH movement and relative clauses, also documented in the literature, leads us as well to conclude that it is not this aspect of the *tough movement* constructions that make them appear to us as complex for children.

The source of the apparent complexity for both Chomsky's subjects and the youngest group in Cromer's study--the primitive rule users--I would claim, is the capacity of the children to retreat to their syntactically sanctioned causative analysis in the experimental contexts.[6] Because children do not move abruptly from this stage to an adult stage, but fall into an intermediate group, using both "subject and object" analyses, and erring in their lexical assignment of the adjectives in both cases, we have no evidence for a general change in the grammar that would effect a complete adult system for all of the adjectives in question. We do, on the other hand have support for a picture of grammatical development that involves more than one grammatical module, and in which an unrelated system--in this case the system licensing the productive causatives--veils our view of another developing system.

We also have an interesting question about the intermediate group that the results in R. Cromer's study of nonsense words raise. He noted that children in this group varied in their responses to the words (sometimes using the lexically present NP as the subject of the infinitive and sometimes using it as the object), even given the contexts in which the words were introduced (cf., (3) above). Children whose responses to the other parts of the experiment paralleled what would be adult responses typically used the syntactic contexts to limit their responses to the nonsense words. The lexically present matrix subject was subject in the complements to *riap*, and the matrix subject was object in the complements to *lorep*. In fact, the most that the examples in (3) can tell one is that both of these two nonce words could belong to the category of *nice*; there is no evidence that excludes this analysis for either of them. The older children, then are, in a sense, jumping to conclusions. In more positive terms, they are presumably forced into this deductive approach that will, in fact, give them the best results; they will, in the worst case, only fail to provide two readings for adjectives of the *nice* variety, but they will not misategorize an adjective. Such a failure is one of the easiest to remedy with the positive evidence available. A question that remains is what distinguishes the child who will not be strongly influenced by the structures in which s/he first encounters the adjectives and the child who will. Given that we know one difference is lexical maturity, we can ask what role that plays in this development.

We have seen that the complexity of *tough movement* clauses may be an issue for us to face, more than it is an issue for children. We have also seen that the difference between children with distinct responses to these constructions may not be a function at all of an overriding primitive rule operating for these structures in particular. Rather, the youngest children's responses internal to the experiments are a function of an independently motivated grammatical system interacting with we can call the pragmatic demands of the experimental situation itself. This grammatical system obscures our view of what these children really know about the complement structure of the adjectives in question, although other evidence shows that very young children are in control of the structure imputed to the *tough movement* constructions. We have also seen that lexical development--measured in terms of vocabulary knowledge in Cromer's study--plays an important role in the movement of children from one stage to another. Although our understanding of this fact remains to be made precise, we are in a strong position to do so, having separated these issues from the general issue of children's analyses of *tough movement* constructions.

NOTES

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[1] Elsewhere I argue that children develop first a grammar with an ergative system working in the syntax, related to a principle such as (11) and its corollary. Children later abandon such a system, moving the analysis of ergativity to the lexicon. (cf. Roeper and Keyser (1985) and Napoli (1987) for differing views of the ergativity system in English.

[2] This was reported in Bowerman's work, attributed to a 3:3 year old child who used it in reference to her inability to make her doll eat.

[3] This account is easier for us to see with some of the adjectives than it is with others. Compare **the duck is fun to bite** and **the duck is tasty to bite**, for example. Nonetheless, given our lack of knowledge about the children's attribution of thematic structure to individual lexical items here, such as **tasty, anxious, creepy**, etc., we should not necessarily be bound by our own adult knowledge of these adjectives. In general, while it is the case that our view of children's developing linguistic systems will be made clearer through the lense of an explicit theory of available linguistic systems, and should be constrained by such a theory, our views of children's underlying linguistic systems must not be exclusively filtered through our understanding of their language only as speakers of its adult version.

[4] The question of what moves a child to recognize passive morphology as the licensing agent for movement insofar as it induces the presence of a VP internal trace confronts us here. This motivation may grow with the recognition of the category into which the English language falls with respect to the interaction of bound morphology and syntax. Addressing some of the issues such a question raises is work by Jaeggli and Safir (1987) and Jaeggli and Hyams(1987). Of course Roeper(1987a, b) deals with questions related to the intersection of bound (derivational) morphology and syntax in this context a well.

[5] See Nishigauchi and Roeper (1985) for discussion of these purpose infinitives(which they have found in the spontaneous speech of a child between the ages of 2 and 31/2), the presence of **for** in them, and the issue of analyzing them as instances of operator movement.

[6] An obvious question is whether children would fall into any "primitive rule user" type group if the context inviting a causative analysis, but making it impossible for us to see explicitly, were removed. An experiment using only pictures depicting three possible interpretations for a sentences such as **the wolf is hard to bite**, including an explicit causative, an analysis with **the wolf** subject of the infinitive, and an analysis with **the wolf** as object of the infinitive is planned. The three categories of adjectives, illustrated in (4) would be included.

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Precocious Passives (and Antipassives) in Quiche Mayan

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The passive construction in English has long had a key role in the development of linguistic theory. This is hardly surprising given the way passives change the mapping between semantic roles and grammatical relations. It is the premiere example of the fact that grammatical relations are not isomorphic with any single semantic role. The role-changing aspect of passives has also meant that studies of its acquisition could potentially determine the extent to which children initially base their grammars on semantic or syntactic categories and relations. Unfortunately, theoretical interest in the acquisition of passives is not matched by parental enthusiasm in providing examples of passive sentences to children learning English. Researchers have had to devise techniques for teaching passives to English-speaking children to see what they could do. Their results reflect both the capacity of children to acquire passives and refinements in the techniques used to elicit responses from young children. There are languages, though, where passive constructions appear fairly frequently in everyday conversation and where children are exposed to passive sentences from birth (cf. Demuth 1988, Savasir 1983, Suzman 1985, 1987). Acquisition studies in such languages may provide new insights into children's capacity for learning grammatical structure.

I have been studying the acquisition of the Mayan language Quiche for some time now. Quiche is spoken by more than a half million people living in the Western Highland region of Guatemala. Sentences in various voices appear in Quiche speech to children, although sentences in the active voice predominate. In this paper I present the morphology of voice marking in Quiche together with data from samples of spontaneous speech and comprehension tests. I also discuss some implications of the Quiche results for current accounts of the acquisition of passives.

1. Quiche Voice Morphology

1.1. Active voice

Two features determine the form of Quiche verbs in the active voice: transitivity and derivation. Root transitive and intransitive verbs are monosyllabic while derived transitive verbs end in a vowel. The general form of these verbs and some examples are shown below:

Aspect-(Obj)-Subj-Root-Termination

Root Transitive	k incomp	- at - 2nd	inw - 1st	il - see	oh Term	'I see you.'
Derived Transitive	k incomp	- at - 2nd	in - 1st	q'aluu-x hug	Term	'I hug you.'
Root Intransitive	k incomp	-	at - 2nd	kam - die	ik Term	'You are dying.'

The agreement markers are obligatory on both transitive and intransitive verbs. An ergative set of person markers is used to mark subject agreement on transitive verbs while an absolutive set of person markers indicates the object of transitive verbs and the subject of intransitive verbs (Pye 1980a). The final suffix on the verbs is the termination marker. The form of the termination marker depends on whether the verb is transitive or intransitive, root transitive or derived transitive, and in clause-medial or clause-final position (Mondloch 1978a, Pye 1983). Thus, the termination marker serves as a key indicator of a verb's transitivity.

1.2. Passive voice

Quiche has two distinct forms of the passive voice (Mondloch 1981). Passive$_1$ adds an intransitivizing marker /-ʃ/ to derived transitive verbs and lengthens the vowel of most root transitive verbs. The resulting verb only marks agreement with a syntactic subject (the logical object), and takes the intransitive forms of the Termination, e.g.

k-in-tsuku-ʃ-ik 'I am looked for.'
k-at-ch'aay-ik 'You are hit.'

Passive$_1$ allows the demoted agent to be expressed obliquely in a phrase headed by the relational noun *-umaal* (similar to prepositions), e.g.

ʃinch'aay r-umaal lal Mari7y 'I was hit by Mary.'

Passive$_1$ does not permit 1st or 2nd person agents to be expressed obliquely in this fashion.

Passive$_2$ adds the intransitivizing marker /-tax/ to both root and derived transitive verbs. The resulting verb only marks agreement with its subject, and takes intransitive Termination forms, e.g.

k-at-tsuku-tax-ik 'You are looked for.'
k-in-ch'ay-tax-ik 'I am hit.'

There is a subtle semantic distinction between the two passives. Passive$_2$ emphasizes the resulting state of the patient or the successful completion of the action. It also allows the demoted agent to be expressed in an oblique phrase headed by the relational noun ***-umaal***. First and second person agents may appear in this phrase with Passive$_2$.

1.3. Antipassive

Antipassive constructions provide a means of emphasizing the role of the subject. In an antipassive the object is demoted to an oblique position or remains unexpressed. Quiche also has two distinct antipassive constructions. The Agentive form adds the intransitivizing suffix /-ow/ to monosyllabic transitive verbs and /-n/ to polysyllabic transitive verbs, e.g.

k-in-tsuku-n-ik 'I look for.'
k-at-ch'ay-ow-ik 'You hit.'

The Agentive voice emphasizes the subject or agent of the action. The verb becomes intransitive, agreeing with the logical subject and taking the intransitive Termination. The Agentive form is obligatory when the agent is advanced by Question formation, Relative clauses, or Focus.
The Agentive must have a subject or object in the 3rd person in its underlying form. Agreement in Agentive verbs follows the person hierarchy: 1,2 > 3 pl. > 3 sing. That is, if one of the actors is a 1st or 2nd person, the Agentive verb will agree with that actor, regardless of whether or not it is the logical subject.

The Absolutive emphasizes the verb's action. It adds the intransitivizer /-an/ to root transitive verbs. The absolutive form of derived transitive verbs is the same as the agentive, e.g.

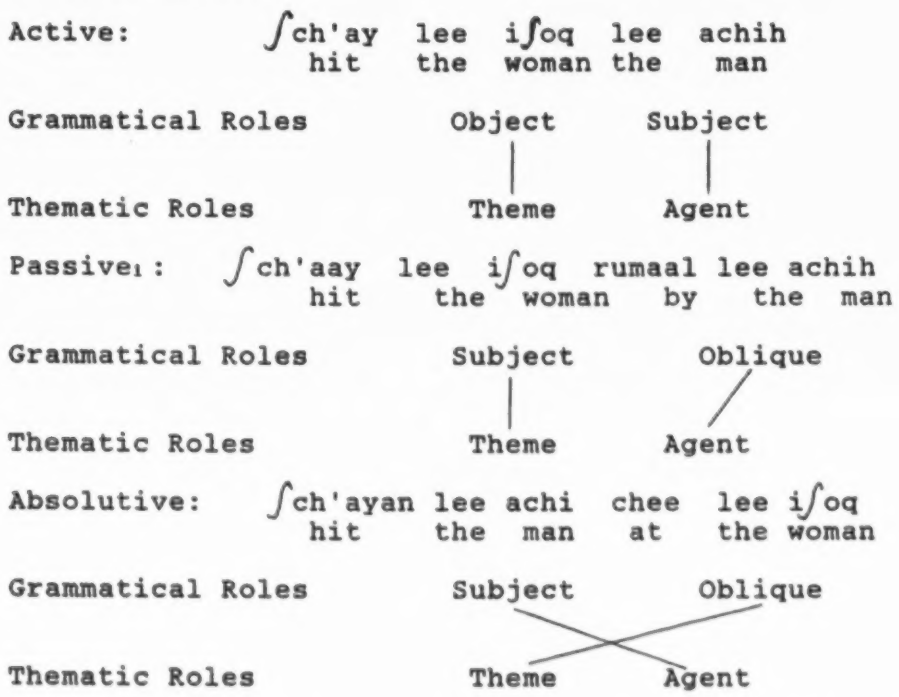
k-in-ch'ax-an-ik 'I wash.' or 'I wash myself.'
k-at-tsuku-n-ik 'You look for.'

Again the resulting verb agrees with the logical subject and takes the intransitive termination. The demoted object may be expressed in an oblique phrase headed by the relational noun ***ch-ee(ch)***, e.g.

ʃ-0-ch'ay-an lee achih ch-ee lee iʃoq
comp.-3A-hit-abs the man on the woman
'The man was hitting on the woman.'

2. Theoretical Digression

Antipassive constructions are regarded as the hallmark of ergative languages, and it is no accident that they appear in Quiche. However, the presence of both passive and antipassive constructions in the same language has interesting implications for theoretical accounts of language acquisition. Consider the effect these constructions have upon the links between semantic roles and syntactic relations. The canonical links between semantic and syntactic roles should be defined by sentences in the active voice. The Quiche passive does not actually involve crossed linkages due to the unmarked word order for active sentences. Thus, if canonical linking rules are defined with respect to the unmarked word order of sentences in the active voice, a simple factor of surface word order should play a significant role in determining the relative ease of passive acquisition across languages. In comparison, the antipassive construction in Quiche requires crossed linkages, so it should be acquired later than the active and passive voices.



Other theoretical approaches would not fair much better. GB accounts of the passive, for example, revolve around the externalization of the object's θ-role (Chomsky 1981). This becomes possible in passive sentences because

the subject's θ-role is absorbed by the passive morpheme and the verb no longer assigns Case to the object position. However, this framework does not provide any insights into antipassive constructions which on the surface would seem to involve the same principles of θ-role absorption and Case assignment. The theory does not explain why the passive morpheme absorbs the subject's θ-role while the antipassive morpheme absorbs the object's. Moreover the framework leads to the same differentiation of passive and antipassive structures. Borer & Wexler (1987), to cite one example in this framework, propose that verbal passives are absent in English children's early speech essentially because they require np-movement. Verbal antipassives, however, do not entail np-movement. Thus, their theory would predict Quiche children should acquire active and antipassive sentences equally easily, and both should appear before passives.

3. Voice forms in Quiche children's spontaneous utterances

Although the overwhelming majority of children's utterances are in the Active voice, they begin using the other voices when they are 2 years old. My data comes from recordings of children's conversations that I made in the course of my dissertation research (Pye 1980b). These data can best be compared with data on passives in English published in an article by Pinker, Lebeaux & Frost (1987). The Quiche and English production data are summarized below:

<u>English (from Pinker, Lebeaux & Frost 1987)</u>

Children	Ages	MLU	Hours Recorded	No. of Passives
Adam	2;3-4;11	2.00-5.20	110	72
Eve	1;6-2;3	1.50-4.26	40	10
Sarah	2;3-5;1	1.74-4.10	139	32
Allison	1;5-2;10	1.73	4	2
<u>Quiche</u>				
Al Tiyaan	2;1-2;10	1.07-3.30	16	19
Al Chaay	2;9-3;6	1.57-4.31	24	99
A Carlos	3;0-3;10	1.59-3.69	20	68

The English data is somewhat exaggerated. Pinker et al. state that they used a very "liberal" definition for passives that included both adjectives (***named***, ***crowded***, ***mixed up***) and possible cases of the simple past tense ('It's stopped in the sky'). In contrast the Quiche data is an underestimate. I have not been able to thoroughly review my

2.

consideration the existence of languages like Quiche which contain both passive and antipassive structures in order to embrace the full range of voice change in human languages.

Finally, the Quiche children are precocious in their production and comprehension of nonactional verbs in nonactive voice. Again the explanation may lie in the structure of the adult language. The results from children learning English show that the children discriminate between the sets of verbs. We do not know whether the basis for this discrimination is one of action versus nonaction, active versus stative or some other yet unknown dimension (Brown [1973] mentions a volitary-involuntary distinction). The nonactional verbs in Quiche, however, are every bit as active as their actional counterparts. It is perfectly grammatical in Quiche to use the progressive aspect and imperative mood with any transitive verb, including want, see, and know. This suggests that the English result indicates children's tendency to erase a distinction that plays a prominent role in the adult language. If this is, in fact, the explanation for the English result, it is an extremely interesting example of children's willingness to overgeneralize a distinction beyond its appropriate domain of application.

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Encounters with Japanese Verbs: Categorization into Transitive and Intransitive Classes

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This paper deals with the acquisition of the syntactic subcategorization of action verbs. By action verbs, I mean verbs that refer to physical transformations of the state or location of an entity. Rather than attempt a broad discussion of the issue, I will concentrate on a particular case, the implications of which are extensive. The particular case is the acquisition of action verbs in Japanese. A and B are examples of fully explicit, and quite atypical action sentences in Japanese:

(A) *Kodomo ga hako o ana no naka ni ire ta*
child NOM wall ACC hole's inside LOC put-PAST
Child put wall inside hole.

(B) *Hako ga ana no naka ni hait ta*
wall NOM hole's inside LOC go-PAST
Wall went inside hole.

Japanese is a nominative-accusative language. In transitive action sentences, semantic causers take nominative case, while semantic figures and patients take accusative case. In intransitive action sentences the semantic figure and patients have nominative case. In active sentences, nominative case is marked by the postposition *ga*, and accusative case is marked by the postposition *o*. Unlike English which has verb pairs such as *open* transitive and intransitive, in Japanese a transitive verb cannot have an homophonous intransitive counterpart. What makes Japanese interesting is that the average causative sentence underdetermines the syntactic classification of its verb. Sentences A' and B' are far more typical of Japanese causative sentences.

(A') *ire ta*
put-REGULAR
(Please) put in (wall inside hole).

(B') *hait ta*
go in-PAST
(Wall) went in (inside hole).

Despite this indeterminacy the Japanese child comes to know that intransitive action verbs like *hair*- "go in" cannot take a causer argument, and that their figure arguments are marked by *ga*. Conversely, the child comes to know that transitive action verbs like *ire*- "put in" can take a causer argument and that their figure arguments are marked by *o*. Accounts of how the child specifies these characteristics of an action verb are not well detailed. Pinker (1989) proposed Direct Learning from Positive Evidence, a mechanism by which the child learns a verb in a sentence with its array of NP arguments, and constructs a "phrase structure tree for the sentence from already

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acquired phrase structure rules" (p. 265). Pinker saw a problem with Direct Learning: it works with preexisting phrase structure rules. The child must first discover logical case instances of subjects and objects in the target language before Direct Learning may start. This is why Pinker proposed that the first lexical entries are filled out by Canonical Mapping, which induces syntactic subcategorization directly from thematic roles. However, to make canonical mapping work, Pinker (1984) argued that the child must "recover thematic roles inherent in the described action" (p. 287). The problem with this hypothesis is that actions do not come labeled "transitive" or "intransitive" in the environment. The child may recover an agent from the context when a verb is intransitive. This point led Bowe and Wexler (1987) to posit the Thematic Inference Principle, which states that, "when a learner is computing thematic roles from structure, s/he assumes a thematic role only if it can be related to an appropriate phrase in the sentence" (p. 135). Thus, when the child hears an intransitive sentence, the child will not associate a causer role with the verb.

These accounts are challenged by the facts of caregiver speech in Japanese. The two-year-old Japanese child seldom hears an input action sentence with the full complement of noun phrases needed to specify the correct number and type of thematic roles for a verb. When NPs are explicit in a sentence, they are seldom accompanied by case marking. To illustrate this point, 300 action sentences were analyzed to estimate the availability of explicit causer, figure and patient NPs, and the availability of case marking for nominative and accusative case (Table 1). Two hundred and fifty of the sentences come from my observations of three boys, H (22-24 months), J (22-24 months), and T, J's older brother (26-30 months). The observations were done in the home, when the children were playing or snacking. Fifty sentences were taken from the transcripts of H's father and mother, J's father and mother and T's babysitter. In addition, 50 more sentences were taken from the published transcripts of a Japanese mother interacting with her son, Taeshan, on his second birthday (Okubo 1983, p. 1-6). The observations of Taeshan and mother were made in contexts similar to my own observations.

The sentences were coded for: 1) Explicit causer, figure or patient NPs, and 2) the case marking of these explicit NPs (Table 1). Pooled results are reported as the individuals all showed the same basic pattern of results. The most common type of intransitive sentence did not have an explicit figure or patient argument (Table 1). The most common type of transitive sentence did not have an explicit causer argument. There were no transitive sentences with case marking on both the causer and figure or patient argument. Generally, when arguments were explicit, they lacked case marking, making it impossible to specify the syntactic relations in a great majority of sentences. The tremendous rate of ellipsis and infrequency of case marking in these caregiver sentences call into question the efficacy of Direct Learning from Positive Evidence (Pinker 1984). So the effectiveness of the Thematic Inference Principle (Bowe & Wexler 1987) is also doubtful. There are so many missing causer NPs, that a child could end up assigning transitive verbs to the intransitive class. Both of these mechanisms labor because of their dependence upon the correct number and type of explicit NPs. Presently, I will outline an alternative account that avoids this dependence.

Recall that Pinker's (1984) position assumes that the child knows a great deal about intentionality, causality, figure-ground relations, and changes of state.

Assuming the same things, an account can be drawn of the acquisition of the transitive and intransitive action verb classes without reference to explicit NPs. This account makes use of semantic causal types such as those discussed by Talmy (1976). Semantic causal types are integrated into the Aktionsart typology used in Role and Reference Grammar (Foley & Van Valin 1984).

By their second birthday children identify the figure or patient of an action nonlinguistically, and they differentiate between entities with intentions and objects without intentions (Golinkoff, Harding, Carlson, & Sexton, 1982). These two sets of distinctions, the perceptual and psychological are orthogonal. Their intersection define of three semantic causal types: self-agentive, causal agentive and non-agentive. The semantic causal types have specialized contexts of use (Figure 1). The self-agentive type is used when the intention to act is attributed to the figure or patient. In order to know that a verb has a self-agentive use, the child must have evidence that such is the case. There must be an animate figure or patient in the context. Additional evidence is the presence of a verb inflection or auxiliary, such as an imperative or desiderative, that implies the intentional origin of an action. Research on the acquisition of morphology in Japanese, indicates that the two-year-old Japanese child understands at least some of these morphemes (Okubo 1967, Clancy 1986, Rispoli 1988). In contrast, to establish that a verb can be used as a causal-agentive, the attribution of intention to the figure or patient must be blocked, as for example when the figure or patient is inanimate. If the child finds a morpheme that implies intention in a sentence with an inanimate figure or patient in the context, the child deduces that there is an intentional causer participant relevant to the meaning of the verb. This deduction occurs without regard to the lexical instantiation of an agent NP. Finally, without evidence from morphology that a verb can express an agentive type, a verb is taken to express the non-agentive type.

Next I would like to illustrate how the use of an action verb in specialized contexts predicts its syntactic classification. The sample of 300 caregiver sentences was coded for 1) figure or patient referent animacy (including implicit figures and patients) and 2) the presence of a verb suffix or auxiliary that implied the intentional origin of an action. Animacy had three levels, 1) true animate beings, 2) animate surrogates (dolls and pictures of animates) and 3) inanimate objects. The suffixes and auxiliaries implying the intentional origin of an action were: *verb+te* "request", *verb+tai* "desiderative", *verb+fyjo* "hortatory", *verb+cha dame* "prohibitional", *verb+te ii* "permissive", *verb+te kure-* and *verb+te age-* "benefactive" (Suga 1983, p. 87-98). The observed contexts for individual verbs were fitted to the array of specialized contexts for each of the three semantic causal types.

Let us take the fitting of the verbs *suwar-* "sit" (intransitive), *ire-* "put in" (transitive) *hair-* "go in" (intransitive) as examples (Figure 2). There were eight examples of the verb *suwar-*, and four of these examples had morphemes that imply intention, in combination with animate figure referents. Since *suwar-* appeared in the expected context for a self-agentive type, and not in the expected context for a causal-agentive type, the verb *suwar-* was matched to the self-agentive type. There were seven examples of the verb *ire-* "put in". Two of the sentences with *ire-* had morphemes that imply intention. All of the sentences with *ire-* had inanimate figure referents. Since *ire-* appeared in causal-agentive context, and did not appear in the context for the self-agentive type, the verb *ire-* was matched to the causal-agentive type. There were

11 examples the verb *hair-*, but none had morphemes that imply intentional action. Therefore, the verb *hair-* was matched to the non-agentive type.

Fourteen other action verbs were fitted in the same manner (Table 2). Only verbs that occurred five times or more were fitted, to reduce the effects of sampling error. All of the transitive verbs were matched to the causal-agentive type. All of the intransitive verbs were matched to either the self-agentive type or to the non-agentive type. In fact, in the larger sample of 300 sentences, none of the intransitive sentences appeared in the causal-agentive context, and none of the transitive sentences appeared in the self-agentive context. These results show that there is information in the input, not contained in either case marking or explicit NPs, that can allow a semantic grouping of action verbs predicting later syntactic classification.

This approach steers a course through two extreme theoretical positions. One extreme holds that children "recover thematic roles inherent in the described action" (Pinker 1984 p. 297). The other extreme holds that a child "assumes a thematic role only if it can be related to an appropriate phrase" (Bowerman & Wexler, 1987 p. 135). Both of these extremes lead to problems. The middle course charted in this paper holds that the two-year-old child assumes that one participant, the figure or patient, is related to an action predicate. The child does not assume that causers in the context are destined to become thematic roles. The child assumes that the intentionality of the figure or patient will be important, and pays attention to inflections and auxiliaries that imply the intention to act. When the child is faced with an inanimate figure or patient in the context, simultaneously with the morphological expression of intention, the child deduces that an agent is relevant to the predicate, even if the agent NP is missing from the sentence. Thus, the child may go beyond explicit NPs.

This account leads to developmental predictions for the acquisition of Japanese. First, young Japanese children should not use intransitive action verbs in the causal-agentive context, nor transitive verbs in the self-agentive context. Second, the acquisition of morphemes that imply intention will proceed more rapidly than the acquisition of the case markers, because of their importance in expressing semantic causal types. Third, since the number and type of arguments a verb takes are related to semantic causal types, then errors in producing the correct number and type of arguments should be fewer than errors in the production of case marking.

As a first check on the plausibility of these predictions the action sentences of two Japanese children were examined, T and H. Each of the children were observed for a three month period: T (28-30 mo.)and H (22-24 mo.). One hour of tape recorded interaction with caregivers was transcribed per month for both boys. The child sentences were coded in the same manner as adult sentences.

T produced 13 verbs five times or more (Table 3). All of T's transitive verbs were matched to the causal-agentive type. All of T's intransitive verbs appeared in the either the self-agentive or non-agentive context. As predicted, there was no use of an intransitive verb in the causal-agentive context. T never produced an intransitive verb with a causer NP. The number and type of arguments were entirely appropriate. As can be seen from Table 1, T was very adult-like in the rarity of case marking. In contrast, 26% (80) of T's sentences had inflections that imply intentional action. The acquisition of morphemes that imply intention proceeded more rapidly than the acquisition of case marking. In fact, twice T unconventionally produced the

nominative case marker on figure NPs of transitive sentences.

There is also evidence of semantic grouping among H's frequently used action verbs (Table 3). None of the intransitive verbs appeared in the causal-agentive context, and none of the transitive verbs appeared in the self-agentive context. H produced neither nominative nor accusative case marking (Table 1). In contrast, H produced 10 inflections that imply intention. All of these inflections were found on transitive verbs. H never produced an intransitive verb with a causer NP. The number and type of arguments produced were always appropriate.

The course of development followed by H and T is consonant with this account. First, these children used intransitive action verbs in the self-agentive context or the non-agentive context, but not in the causal-agentive context. They used transitive verbs in the causal-agentive context, but not in the self-agentive context. Second, the acquisition of inflections and auxiliaries that imply intention proceeded more rapidly than the acquisition of the case marking. Third, they did not produce explicit causers with intransitive verbs, so that the number and type of arguments which a verb took were always appropriate. In contrast there were two case marking errors in the older child's sample. Such errors have also been reported by Clancy (1986), but to my knowledge errors in the number and type of arguments have never been reported. Taken individually, none of these observations provide strong support for the present hypothesis. However, taken together, in conjunction with the facts of Japanese input, this account of the acquisition of action verb subcategorization seems quite plausible.

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Table 1

Frequency of Explicit Causer, Figure or Patient NPs and Case Marking in the Action Sentences of Japanese Caregivers and Two Chidren

	Caregivers		Children			
Sentence Type			H (22-24 mo.)		T (28-30 mo.)	
Intransitive Sentences	Freq.	%	Freq	%	Freq	%
- figure / patient NP	97	.63	91	.92	77	.71
+ figure / patient NP						
- *ga*	50	.32	8	.08	27	.25
+ *ga*	8	.05	0	.00	4	.04
Total	155	1.00	99	1.00	108	1.00
Transitive Sentences	Freq.	%	Freq.	%	Freq.	%
- causer, - figure / patient NP	44	.30	32	.56	44	.38
+ causer NP only						
- *ga*	19	.13	20	.35	6	.05
+ *ga*	6	.04	0	.00	1	.01
+ figure / patient NP only						
- *o*	48	.33	3	.05	51	.44
+ *o*	9	.06	0	.00	4	.03
+ causer, + figure / patient NPs						
- *ga*, - *o*	15	.10	2	.04	6	.05
+ *ga*, - *o*	3	.02	0	.00	4	.03
+ *o*, - *ga*	1	.01	0	.00	0	.00
+ *ga*, + *o*	0	.00	0	.00	1	.01
Total	145	1.00	57	1.00	117	1.00

Note: *ga* = nominative case marker, *o* = accusative case marker

Figure 1

Expected Contexts for Semantic Causal Types

Morphology	Figure or Patient Animacy: Animate	Inanimate
Intentional	Self-Agentive	Causal Agentive
Non-intentional	Non-Agentive	Non-Agentive

Figure 2

Observed Contexts of *hair-* "go in" (intransitive) *suwar-* "sit" (intransitive), and *ire-* "put in" (transitive) in Caregiver Sentences

Morphology	Figure or Patient Animacy: Animate	Inanimate
Intentional Examples:	*suwar-* (intransitive) *suwar-i-tai* "Want to sit" (desiderative)	*ire-* (transitive) *ire-te* "Put in" (request)
Non-intentional Examples:	*suwar-* (intransitive) *suwat-ta* "Sat"	*ire-* (transitive) *ire-ta* "Has put in"
Examples:	*hair-* (intransitive) *hair-u* "Will go in"	*hair-* (intransitive) *hait-te iru* "Is in"

Table 2

Observed Contexts of Verbs
Used Five Times or More By Caregivers

Verb Type	Observed Contexts		
Transitive Verbs	Causal-Agentive	Self-Agentive	Non-Agentive
tor-'take'	y	0	0
age- 'give'	y	0	y
ire- 'put in'	y	0	y
kake-'hang'	y	0	y
tabe-'eat'	y	0	y
nom-'drink'	y	0	y
mot-'hold'	y	0	y
Intransitive Verbs			
ik-'go'	0	y	y
kaer-'come back'	0	y	y
ne-'go to sleep'	0	y	y
suwar-'sit'	0	y	y
hair-'go in'	0	0	y
hashir-'run'	0	0	y
koware-'break'	0	0	y
shin-'die'	0	0	y
tore-'come off'	0	0	y
tsuk-'stick to'	0	0	y

Table 3

Observed Contexts of Verbs Used Five Times or More
By Children H and T

Child H's Verbs

Verb Type	Observed Contexts		
Transitive Verbs	Causal Agentive	Self-Agentive	Non-Agentive
nom-'drink'	y	0	y
tsukur-'make'	y	0	y
kak-'write'	0	0	y
mot-'hold'	0	0	y
Intransitive Verbs			
de-'go out'	0	0	y
hair-'go in'	0	0	y
ik-'go'	0	0	y
koware-'break'	0	0	y
mawar-'turn'	0	0	y
tomar-'stop'	0	0	y
tat-'stand'	0	0	y

Child T's Verbs

Verb Type	Observed Contexts		
Transitive Verbs	Causal-Agentive	Self-Agentive	Non-Agentive
har-'spread'	y	0	y
ire-'put in'	y	0	y
kak-'write'	y	0	y
mot-'hold'	y	0	y
tabe-'eat'	y	0	y
tor-'take'	y	0	y
tsuke-'put on'	y	0	y
Intransitive Verbs			
ik-'go'	0	y	y
kaer-'come back'	0	y	y
ne-'go to sleep'	0	y	y
nor-'ride'	0	y	y
hair-'go in'	0	0	y
tomar-'stop'	0	0	y

Note: y = appeared in context at least once, 0 = did not appear in context

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LANGUAGE LEARNABILITY AND EMPIRICAL PLAUSIBILITY
Null Subjects and Indirect Negative Evidence

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In this paper the linguistic framework assumed is Chomsky's principles-and-parameters theory (e.g. Chomsky 1981, 1986a). First I outline a reanalysis of the null subject parameter; this reanalysis is meant to be illustrative rather than definitive. Then I examine the parameter to determine its learnability, taking Wexler and Manzini's set-theoretical approach (Manzini and Wexler 1987, Wexler and Manzini 1987) as a first approximation to a theory of parameter fixation.

1. Reanalysis

In what follows I assume, with little further comment, that pro-drop and postverbal subjects originate from separate, if not entirely independent, parameters (cf. Hyams 1986, Safir 1985). Obviously, most alleged consequences of pro-drop, e.g. lack of *that*-trace effects and long-distance movement of subjects, can be demonstrated to follow from the inversion of subjects, rather than from pro-drop (cf. Rizzi 1982, but see Picallo 1984 for a contrary view). I further assume, following Chomsky (1986b) and others, that a requirement of proper government (i.e. ECP) does not hold of the pronominal empty category *pro*, which is standardly supposed to occupy the lexically null subject position in pro-drop languages (e.g. Rizzi 1982, Chomsky 1982). Finally, Case, rather than Agr, is considered to be the factor licensing a null subject (Rizzi 1986, Safir 1985); the function of rich inflection is hypothesized to be the identification of the grammatical features of a null subject, a process that may determine whether the latter will be realized in a *particular* language. (See Saleemi, in preparation, for further discussion on these matters.)

Rizzi (1986) argues that licensing and identification of content are characteristically independent processes in the theory of grammar (also see Rizzi 1982). To illustrate, PRO is licensed by the relevant clause of the Generalized Empty Category Principle (Chomsky 1981), but it acquires its content by means of control mechanisms. Likewise, NP-trace and *wh*-trace are sanctioned by the ECP (by lexical government, to be precise), whereas it is through the formation respectively of A-chains and A'-chains that their referential content is fixed. In general, licensing is predominantly syntactic and is exercised through a government-type relation; on the other hand, the recovery of content usually takes place on the LF side, by means of a binding-type relation. In a similar vein, Aoun et al. (1987) argue that the ECP, viewed as a disjunctive condition consisting of a lexical government component and a coindexing component, is a spurious generalization

redundantly combining two distinct types of locality requirement; one structural and operative at PF, the other indexical and applicable at LF.

Interestingly, the null subject parameter has been cited as evidence both for and against Wexler and Manzini's subset principle (op. cit.). It has been claimed that, in the relevant respect, a language generated by the pro-drop option is the superset of a language generated by the non-pro-drop option (e.g. see remarks in Berwick 1985:291-293). Conversely, it has been argued that the two types of languages intersect, since a pro-drop language does not have sentences with overt expletive (or pleonastic) subjects, whereas a non-pro-drop one does not contain sentences with null subjects (Hyams 1986). However, it seems that neither of the two claims is valid, as a binary-valued formulation of the parameter is probably not descriptively adequate.

First consider the following German examples.

(1) a. Heute regnet *(es)
today rains
'it's raining today'
b. Heute sind (*es) zwei Kinder gekommen
today are two children come
'today there came two children' (Travis 1984)

Unlike Italian and Spanish, in German pro-drop is available only in a very reduced range of contexts. Referential pro-drop is not allowed at all. A nonargument subject (a pleonastic that is construed with a postverbal NP or S), *ceteris paribus*, is obligatorily omitted in many constructions (1b), but a quasi-argument subject (the pleonastic subject of atmospheric-temporal predicates; see Chomsky 1981) must always be retained (1a). In other words, in German argumental subjects must never be omitted (Travis 1984; also see Safir 1984, 1985).

According to Travis (1984) Yiddish represents still another type, as these examples show.

(2) a. Haynt hot *(es) alts gegesn
today has it all eaten
'it has eaten everything else today'
b. Haynt geyt (*es) a regn
today goes rain
'it's raining today' (Travis 1984)

As in German, referential pronouns are never dropped in Yiddish (2a). But no nonreferential pronouns, including quasi-arguments (2b), can appear overtly. With respect to nonreferential drop, Malagasy (Travis 1984) and Icelandic (Rizzi 1986) pattern with Yiddish.

Finally, Italian permits referential pro-drop as well as nonreferential pro-drop, as demonstrated below.

(3) a. (Io) vado al cinema
(I) go to the movies
'I go to the movies'
b. Sembra che Gianni sia molto infelice oggi
seems that John is very unhappy today
'it seems that John is very unhappy today'
c. Piove molto durante il mese di febbraio
rains a lot during the month of February
'it rains a lot during the month of February' (Hyams 1986)

Clearly, a revision of the traditional view of pro-drop is in order. Consequently, in part following some typological observations of Rizzi (1986) and Travis (1984), the parameter is reformulated below as a multi-valued parameter. Before introducing the reformulation of the null subject parameter, the following Case Assignment Principle is adopted (cf. Fabb 1984:43).

(4) *Case Assignment Principle*
An NP must be assigned Case at some level (other than D-structure).

This principle makes it possible for an NP to be Case-marked either at S-structure or at LF. It covers the assignment of Case to null NPs as well as overt NPs, the former acquiring Case only at LF. A key idea, due to Bouchard (1984), is that in pro-drop languages Case assignment can be optionally delayed until LF. Essentially, this idea presupposes that obligatory Case at S-structure requires an NP to be lexically realized at PF, whereas in the event of optionality of syntactic Case an NP need not be so realized, unless some other feature (e.g. focus) forces it to acquire Case in order to be overt. The null subject parameter can now be stated as follows.

(5) *Null Subject Parameter*
The assignment of Case to σ may be delayed until LF; where σ, σ a subject, represents
(a) $\emptyset$; or
(b) nonargument; or
(c) nonreferential argument; or
(d) any argument whatsoever.

English and French are associated with value (a) of the parameter, allowing no null subjects. On the other hand, German takes value (b), that permits only nonarguments to be omitted, requiring all argumental subjects to be lexically expressed. Yiddish, Malagasy and Icelandic take value (c), that allows the omission of quasi-

arguments as well as nonarguments, ie all nonreferential subjects. Finally, languages like Italian and Spanish (and possibly also Chinese and Japanese) are associated with value (d), under which any subject, referential or nonreferential, may remain null.

The null subject parameter as stated in (5) gives rise to two related problems, a descriptive problem and a learnability problem, to which I now turn.

2. Descriptive Adequacy

First the descriptive problem. The parameter predicts that pleonastic pro-drop will be optional, just like referential pro-drop. However, whereas referential pro-drop is optional in general, nonreferential pro-drop seems to be mandatory in most pro-drop languages, with a few exceptions, e.g. Irish (Travis 1984:231ff) and Welsh. Practically, then, the pro-drop option with respect to pleonastics might be no more than a Hobson's choice. But there appears to be a simple solution to the problem.

In the spirit of Chomsky's (1981) Avoid Pronoun Principle, the absence of lexical pleonastics in pro-drop languages can be attributed to their pragmatic infelicity. It can be assumed that since pleonastic subjects are nonreferential, they would be superfluous in a pro-drop language. Syntactically, if the pro-drop property indeed springs from the optionality of Case at S-structure, then the avoidance of pleonastic elements is understandable as they will tend not to appear in syntax merely as a spell-out of Case. This will then account for the lack of expletive subjects in most pro-drop languages, and one could still claim (5), pragmatically qualified, to be formally correct. In any event, this maximally general statement of the parameter may be required for languages in which nonreferential pro-drop is in fact optional in many configurations. Nevertheless, due to the irregular distribution of pleonastics in languages, the shortfall in the data available to the learner leaves us with a learnability problem, particularly in the total absence of any kind of negative evidence, as is demonstrated in the following section.

3. Learnability

The problem is that the data the child will get are not exactly the data it will expect under the parameter (5). It is plainly evident that in principle the four values of the parameter should generate languages (i.e. sets of sentences) which form a subset hierarchy, as each value increases the set of well-formed structures allowed by the parameter. This would be compatible with the monotonic model of parameter fixation proposed by Wexler and Manzini (op. cit.), indicating that the parameter could be straightforwardly fixed on the basis of positive-only evidence. However, due to the nonappearance of expletives in many cases we are left with a rather truncated hierarchy, excluding the subset principle as an effective learning procedure. Nonetheless, it is

still possible to consider that the values of the parameter are ordered in terms of markedness just as dictated by a subset hierarchy, as in theory the parameter is compatible with the subset principle. Equivalently, one can define the inclusion relations among values, rather than extensionally (i.e. among languages generated under these values), a possibility that follows naturally from the parameter. The markedness hierarchy and the learning procedure can then be defined accordingly (see Saleemi, in preparation, for definitions).

Recall that the set of null subject types under the four values progressively enlarges from value (a) to value (d): the set of null subjects under value (a) of the parameter is Ø; the set of possible null subjects under value (b) consists of nonarguments only; the set of possible null subjects under value (c) has as its members quasi-arguments as well as nonarguments; and the set of possible null subjects under value (d) contains nonarguments, quasi-arguments, and referential arguments. So, in this specific sense value (d) includes value (c), value (c) includes value (b), and value (b) includes value (a); that is, given the parameter P (= 5) with values ranging over P_a, ..., P_d, $P_a \subseteq P_b \subseteq P_c \subseteq P_d$.

The intuitive idea is that markedness is a function of certain internal properties of language, rather than of the external properties of particular languages (cf. Chomsky 1986a). However, it should be stressed that the data the child utilizes to instantiate parameters can only be described in terms of languages, an example of the intricate connection between parameter values and the corresponding languages. So let us now consider these languages and the set-theoretical relationships between them.

Let L(a) be the language generated by value (a) of the parameter; likewise L(b), L(c), and L(d). Then it is obvious that L(a) and L(b) intersect, as L(a) contains sentences with overt nonarguments, which L(b) does not have, and L(b) contains sentences with null nonarguments that are excluded by L(a). Next consider L(c). L(c) has sentences with null quasi-arguments, not included in L(b), whereas L(b) has sentences with overt quasi-arguments that are not contained in L(c). Therefore L(b) and L(c) also intersect. Now consider L(d). L(d) is coextensive with L(c) with respect to the nonpresence of nonreferential subjects, but it additionally contains referential null subjects. Consequently, $L(c) \subseteq L(d)$.

In short, the set-theoretical profile that emerges is rather mixed, incorporating *both* subset and intersecting relations, quite unlike what the subset principle and the related assumptions predict. The following consequence immediately ensues: if the correct language is any language other than L(a), then its selection might lead to overgeneralization *within* that language.

Suppose L(b) is the ambient language. Recall that in L(b) all argumental subjects must be overt. Then, when presented with

sentences with null nonarguments, the learner is bound to conjecture the parametric identity of the language. But notice that, given the no-negative evidence assumption, there is nothing that would prevent him from considering sentences with overt nonarguments to be in L(b), as exemplified here with respect to German.

(6) *Heute sind es zwei Kinder gekommen

Now consider that L(c) is the language to be learned. The presence in the data of sentences with both kinds of null nonreferential subjects should be sufficient to rule out L(a) and L(b), pointing to L(c) as the most likely choice. But the learner might still erroneously regard sentences with overt quasi-argument subjects (see the Yiddish example in 7), as well as those with overt nonargument subjects, to be in L(c).

(7) *Haynt geyt es a regn

Likewise in the case of L(d). The appearance in the data of null referential subjects should straightaway rule out L(a), L(b), and L(c). But the problem of possible overgeneralization to overt pleonastics within L(d) is still there. Considering that quite often expletives are homonymous with certain referential pronouns (e.g. Yiddish *es* and English *it* have referential analogues; notably, Welsh *hi* is 3rd person feminine singular), in principle overgeneralization can occur even though overt expletives are totally absent in the language being learned, as they are in Italian. In sum, it seems that although positive-only evidence is effective in *positively identifying* a language from among the four possible ones, it is not effective in *exactly identifying* that language.

One way out might be to posit that the child learner is able to undergeneralize within the conjectured language, by noticing the nonoccurrence of certain types of overt pleonastic subjects in the 'incomplete' data, in other words to resort to what Chomsky (1981) called indirect negative evidence (also see Lasnik, to appear, Oehrle 1985). Clearly, the use of indirect negative evidence can indeed exactly identify the correct value of a parameter from incomplete data.

It is possible to argue that indirect negative evidence is not really necessary, since alternative accounts based on positive-only evidence are available. For example, one can argue that the constraint stated in (8) is a part of the learner's *a priori* baggage, which stipulates the absence of redundant forms such as expletive subjects unless they are observed in positive data.

(8) *Redundancy Constraint*
Assume redundant forms to be absent unless they are exemplified in positive data.

This constraint, comparable but not identical to the Avoid Pronoun Principle, would make it possible for the parameter to be fixed from positive-only evidence. But there are a number of reasons why such a move might be ill-advised.

The constraint in question has no independent empirical status whatsoever, its only justification being that it salvages the 'no-negative evidence' condition; so it acts merely as a preemptive substitute for some form of (implicit) negative data. A major danger in stipulating such constraints is that it is all too easy to go on adding them to the learning system; the resulting interplay between parameters and constraints is very likely to become too undisciplined. Further, the constraint is essentially no more than a codified quasi-pragmatic tendency. I believe there are compelling methodological reasons for postulating that grammatical and pragmatic phenomena should never be conflated, since a maximally simple linguistic theory will be obtained by excluding these latter from the characterization of Universal Grammar.

Thus, it seems parsimonious to assume that the parameter is as stated in (5), and the extensional shortfall in the languages is due to pragmatic factors, not to be accounted for in the theory of grammar or learnability. The alternative to (8) (or similar *ad hoc* assumptions), of course, is to hypothesize that *some* use of indirect negative evidence is legitimate, so that the learner assumes pleonastics to be present unless they fail to show up in the data. Of course positive evidence must still retain its primary role, with indirect negative evidence supplementing this role only if the need to exclude some consequences of a parametric choice arises. So it is proposed that in general the following constraint holds of the human language learning system.

(9) *The Indirect Negative Evidence Constraint*
On no account can the choice of a parameter value, or a change in the value of a parameter, be made solely on the basis of indirect negative evidence.

4. Conclusion

To sum up, the relationship between parameter values and the languages they generate cannot invariably be captured extensionally in terms of simple set-theoretical relations, perhaps suggesting that parameters do not, strictly speaking, generate languages, but only fix the maximal bounds within which languages can be realized. Further, under the linguistic theory assumed the induction of a natural language is increasingly viewed as a search in a radically circumscribed choice space, rendering

it psychologically plausible that the child may be sensitive to nonoccurrence of certain types of data, a step which would be untenable under a less restrictive linguistic framework. Thus, in the end, the view of learnability just described may be empirically plausible only to the extent that the theory of principles and parameters is empirically plausible.

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PRCLD 27 (1988)

The Acquisition of Reflexives and Pronouns by Icelandic Children

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In this paper we report the results of an experimental study on the interpretation of lexical anaphors and pronouns by Icelandic-speaking children. In recent years there have been many studies on children's acquisition of lexical anaphors and pronouns. Most of these studies have been concerned with English-speaking children, for example, Wexler & Chien (1985); Chien & Wexler (1987a) ; Otsu (1981); Jakubowicz (1984); Solan (1987); McDaniel, Cairns & Hsu (1987), but more recently studies have been conducted on children acquiring other languages, for example, Chinese (Chien and Wexler 1987b), Korean (Lee and Wexler 1987), Dutch (Koster and Koster 1986) and Italian (Crain and McKee 1987). The cross-linguistic study of the development of binding in children has proceeded in tandem with research on binding in adult languages. In the course of the investigation of adult languages it has been revealed that not all languages obey the same binding conditions as English. A case in point are the so-called long-distance reflexives found in various languages, such as Icelandic, Chinese and Korean. Languages which have binding properties that are distinct from English are of particular interest for linguistic theory as these languages seem to challenge the standard binding theory of Chomsky (1981).

The standard binding theory, as introduced in Chomsky (1981), consists essentially of two principles, principles A and B which can be informally reformulated as in (1):

(1) Principle A: An anaphor must be locally bound
Principle B: A pronoun may not be locally bound

where the term 'bound' means 'c-commanded by and coindexed with its antecedent' and for our present purposes 'local' means within the same clause.

The standard binding theory in (1) correctly accounts for English anaphors and pronouns, as illustrated in (2), and indeed for anaphors and pronouns across a number of different languages.

(2) a) John shaves himself
b) *John told Bill to shave himself
c) *John shaves him
d) John told Bill to shave him

(underlined NPs are coreferent)

However, there are languages with reflexives and pronouns that cannot be correctly accounted for by the standard binding theory. Icelandic is one of the languages that has binding properties which are different from English. The following is a brief description of the Icelandic facts. The reflexive element sig in Icelandic is equivalent in meaning to English himself/herself. Sig is a 3rd person form which is invariant for gender and number, but which has three different case forms as illustrated in (3):

(3) Sig = himself/herself
3rd pers. m./f./n. sg./pl. acc. sig
dat. sér
gen. sín

As first outlined in Thráinsson (1976a,b), the reflexive element sig in Icelandic can have a non-local, that is "long-distance antecedent." When sig occurs in a single clause sentence, it behaves just like himself/herself in English, that is, it adheres to principle A in (1). This is illustrated in (4).

(4) Jón rakar sig
John shaves himself

The difference between Icelandic sig and English himself/herself shows up in sentences with complement clauses. Here we find that sig may take a long-distance antecedent when the clause that contains sig is subjunctive or infinitive. However, if sig is contained in an indicative clause, it can normally only refer to the local antecedent. There is an additional requirement in Icelandic which is that a long distance antecedent must be a subject, but this need not concern us here. Consider the sentences in (5)-(7):

(5) Kermit$_i$ segir að Jón$_j$ gefi$_{(subj.)}$ sér$_{i/j}$ bíl
Kermit says that John gives sér a car

(6) Kermit$_i$ segir Jóni$_j$ að gefa$_{(inf.)}$ sér$_{i/j}$ disk
Kermit tells John to give sér a plate

(7) Kermit$_i$ sér að Jón$_j$ gefur$_{(ind.)}$ sér$_{*i/j}$ flautu
Kermit sees that John gives sér a whistle

The complement clauses in sentences (5) and (6) are subjunctive and infinitive clauses, respectively. Thus, in these sentences, sér, which is the dative form of the reflexive element sig, can either have the local subject John or the matrix subject Kermit as its antecedent, as is indicated by the subscripts. In contrast, when the complement clause is an indicative clause, as in sentence (7), sér can normally only take the local subject John as its antecedent.

Turning now to Icelandic pronouns, honum which corresponds to English him and henni, which corresponds to

English her, both obey principle B of the binding theory, as given in (1). Thus, Icelandic pronouns may not be locally bound. Thus, the sentence in (8) is just as ungrammatical in Icelandic as it is in English:

(8) *Jón rakar hann
John shaves him

However, there is a difference between the binding properties of Icelandic and English pronouns. When the Icelandic pronoun is contained in a subjunctive or indicative complement clause, it can refer to a NP in the higher clause, as well as to an extra-clausal NP, as is the case in English. However, if the pronoun is contained in an infinitival complement, speakers' intuition is that it cannot refer to the subject of the higher clause. Consider the sentences in (9)-(11):

(9) Svínka$_i$ segir að Sara$_j$ gefi$_{(subj.)}$ henni$_{*j/i/k}$ bíl
Miss Piggy says that Sarah gives her a car

(10) Svínka$_i$ sér að Sara$_j$ gefur$_{(ind.)}$ henni$_{*j/i/k}$ bíl
Miss Piggy sees that Sarah gives her a car

(11) Svínka$_i$ segir Söru$_j$ að gefa$_{(inf.)}$ henni$_{*j/*i/k}$ bíl
Miss Piggy tells Sarah to give her a car

In sentences (9) and (10), which have complement clauses in the subjunctive and indicative moods, respectively, the pronoun can have either the matrix subject Miss Piggy or some extra-clausal NP as its antecedent. In contrast, when the pronoun is contained in an infinitival clause, as in sentence (11), there is a strong preference for it to refer to some extra-clausal NP rather than the matrix subject. There are a number of other properties of Icelandic anaphors and pronouns but these basic facts suffice for our purposes.

To account for the observed variation between languages, Yang (1984) and Wexler and Manzini (1987) propose a parameterized binding theory. According to Wexler and Manzini, the locality condition in principles A and B is a parameter which can be reformulated roughly as in (12):

(12) a local clause contains the anaphor or pronoun and;

a) has a subject; or
b) has an INFL; or
c) has a Tense; or
d) has an indicative Tense; or
e) has a root Tense

On Wexler and Manzini's account English anaphors and pronouns would be associated with value (a) of the parameter, whereas the Icelandic reflexive would be associated with value (d). The Icelandic pronouns take value (a) and there are other languages which pick out the remaining values.

In order to account for how the child ultimately arrives at the correct parameter setting, Wexler and Manzini (1987), following Berwick (1982), propose the Subset Principle, a learning algorithm which seems to be a necessary condition to assure learning without negative data. The Subset Principle is given informally in (13):

(13) The learning function maps the input data to the value of a parameter which generates a language:
a) compatible with the input data, and
b) smallest among the languages compatible with the input data

According to this principle the child hypothesizes the smallest language compatible with the data. Wexler and Manzini further propose that the value which generates the smallest language constitutes the default or unmarked setting of the parameter, hence the one that all children should start out with and one which may be later revised on the basis of positive evidence. Incorporating the Subset Principle into a developmental theory, we have a very explicit prediction regarding the development of the binding module. For anaphors, a grammar that only allows local binding defines a smaller language than a grammar which licenses long-distance binding. Hence, we expect that all children will start out by assuming local binding for reflexives; that is, value (a) of the locality parameter in (12) will be the child's first assumption, even in the case of languages where the grammar licenses long-distance binding such as Icelandic. Our experiment was designed to test the hypothesis that Icelandic children would initially bind the reflexive sig only to its local antecedent.

We tested 120 Icelandic children between the ages of 2;0-6;0, and 15 adult controls on anaphor resolution in 3 sentence types, sentences with indicative, subjunctive and infinitival complements. Examples of the sentences used are given in (5)-(7) and (9)-(11) (above). The names John and Sarah were replaced by the name of the child who was being tested. We used an act-out task, the Party Game, which was developed by Chien and Wexler (1987a), in which the child is asked to perform an action given in a sentence. For example, the child is given the sentence "Miss Piggy says that John gives sér a truck," and has to select a truck from several toys on the table and give it either to himself or to one of four dolls present. The children were divided into 7 groups of six-month intervals based on their ages. Each group included 15 subjects.

The experimental results with sentences containing the anaphor sér are represented in figures (1)-(3) on the following page. In each figure the age group is listed along the abscissa and the frequency along the ordinate. The line with squares indicates coreference with the child, that is, a local antecedent response; the line with crosses indicates coreference with the doll mentioned in the sentence - the long distance antecedent; the line with diamonds indicates

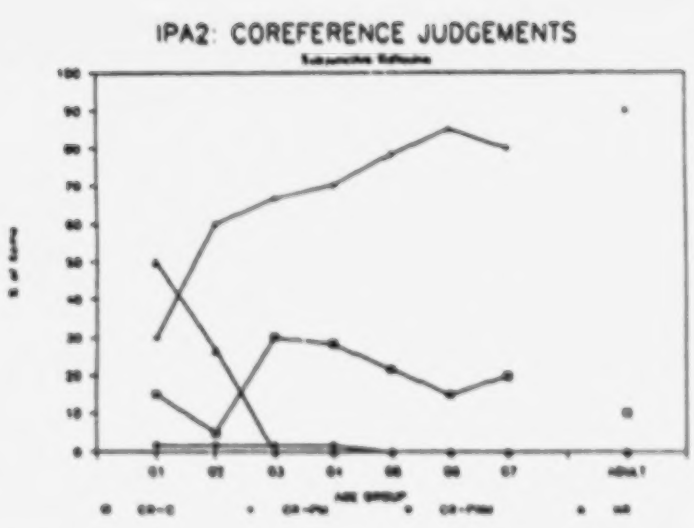

Figure 1 Subjunctive Reflexive

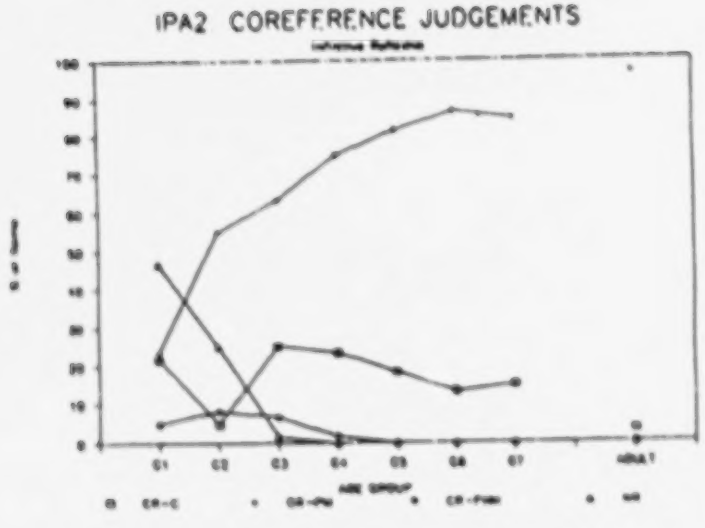

Figure 2 Infinitive Reflexive

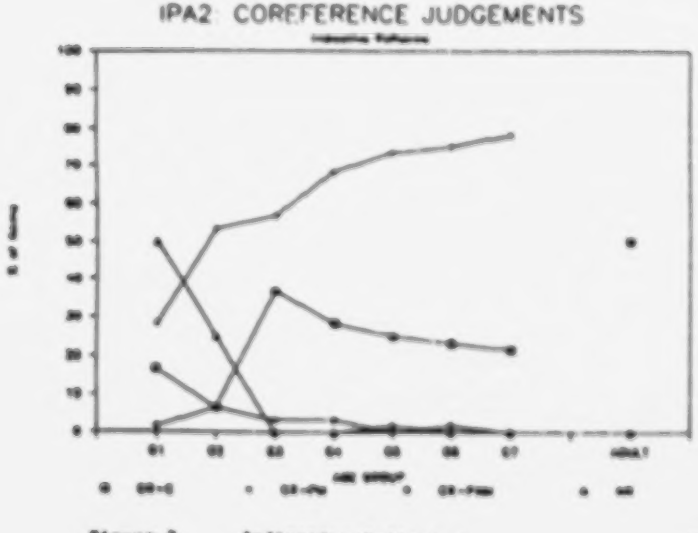

Figure 3 Indicative Reflexive

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coreference with the doll not mentioned - an outside NP; and the line with triangles indicates no -response. The results show that Icelandic children consistently prefer the long-distance antecedent for the reflexive over the local antecedent. F (1,98) = 96.48, $p<.01$. Even the youngest children prefer the long distance antecedent, although many of the 2;6-3;6 year olds, represented by G1 and G2, fail to respond. Interestingly, long distance responses predominate across all three sentences types- subjunctive, infinitive and indicative - despite the fact noted in sentence (7) that when the reflexive sig is contained in a indicative clause, only a local antecedent is judged grammatical.

Notice, however, that the children are not alone in making this "mistake" as the adults also allow sig to refer to the long-distance antecedent 50% of the time in the indicative sentences. We believe that there are two factors which contribute to this particular result. First, for many speakers indicative complements to semifactive verbs like sja "see", behave like subjunctive clauses in allowing long distance antecedent. Thus, some speakers find the sentence in (7) grammatical when the reflexive ser refers to the long-distance antecedent (see for example Sigurdsson (1986). Unfortunately, we were unawere of this dialect variation when we designed the test sentences. It is likely that among the children tested there were at least some speakers of this less restrictive dialect. In addition, it is probable that during the early stages children do not distinguish the different moods and hence their grammar would fail to show the restriction against long distance binding in indicatives. A Wilcoxon Signed Ranks Test for Matched Pairs indicates that the trend of children's long distance responses to indicative sig sentences is significantly different from their long distance responses to subjunctive sig sentences ($p = .02$) and to infinitive sig sentences ($p = .03$), while the difference in long distance responses between subjunctives and infintives is not significant. This is consistent with the claim that at a relatively early age children do sort out the complex constraints on the long distance control of sig.

In summary, the Icelandic children and the adults strongly prefer a long-distance antecedent for the reflexive and this preference gets stronger as the children get older. Thus, the Icelandic children appear not to adhere to the prediction of the subset principle, since they do not initially assume local binding for the reflexive. This result is in marked contrast to the results obtained by Chien & Wexler (1987b) for Chinese and by Lee and Wexler (1987) for Korean. Those studies showed that the children strongly preferred the local antecedent even though these languages allow the reflexive to have a long distance antecedent. The strongest support for the subset principle comes from Lee and Wexler's study on Koerean, where children older than 4;6 preferred the local antecedent almost 100% of the time, whereas the adults preferred a long-distance antecedent about 62% of the time. Chien and Wexler's results on Chinese were consistent with the subset principle, but not supportive of

it since the adults preferred the local antecedent just as strongly as the children.

Thus, Icelandic children seem to exhibit a different developmental pattern from both Korean and Chinese children, although all three languages appear to have a similar type of reflexive, one which allows both a local and a long-distance binding in certain contexts. How is this difference to be accounted for? One apparent non-grammatical explanation for this difference is that our experiment biased the children towards a long-distance response by using the verb give in all the test sentences. Under the assumption that children think it is more natural to give something to someone other than themselves, the results would show a preponderance of long distance responses. However, this explanation cannot be maintained in light of the Chinese results noted above. This study adopted precisely the same experimental design as was used with the Icelandic children - the Party Game using only the verb give, and the Chinese children overwhelmingly preferred the local antecedent, that is, themselves.

The explanation that we want to propose for our results and for the differences obtained between children acquiring Korean and Chinese is that the problem does not lie with the Subset Principle nor with any other aspect of acquisition theory, but with differences in the target grammars. Thus, we want to argue that Icelandic does not in fact have a long-distance reflexive and that sig is actually a bound variable analogous to his in the English sentence in (14a):

(14) a. Everybody loves his mother
b. Everybody hopes that his mother is happy

Bound variables, unlike anaphors, typically enter into long distance dependencies as illustrated in the sentence in (14b). Although the details of this analysis would take us too far afield (but see Hyams & Sigurjónsdóttir, in preparation) it is independently motivated by a number of properties of adult Icelandic. More to the point, it allows us to explain why the Icelandic children behave differently from the children in the other studies, where we would argue that the elements being tested are anaphors, and hence are locally bound by the children as predicted by the Subset Principle. Thus, the Icelandic results are not directly relevant to the acquisition of principle A of the binding theory nor to the Subset Principle. What our results do tell us is that Icelandic children acquire the knowledge of how to handle the bound variable sig, very early in their linguistic development. In addition, the analysis indicates that Icelandic does not provide evidence for a parametrized binding theory, contrary to current assumptions. To the extent that the standard, non-parametrized binding theory can be maintained it is to be preferred since it simplifies linguistic theory and consequently the acquisition task.

Turning to the results of the pronoun sentences, we see in figures (4)-(6) on the following page that the Icelandic

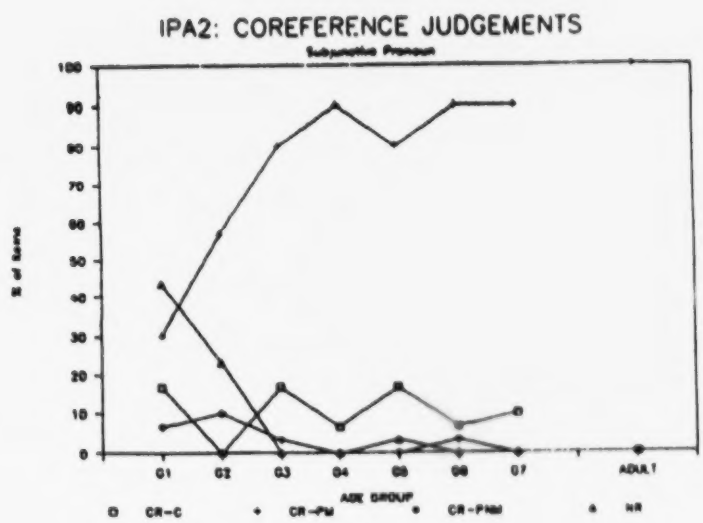

Figure 4 Subjunctive Pronoun

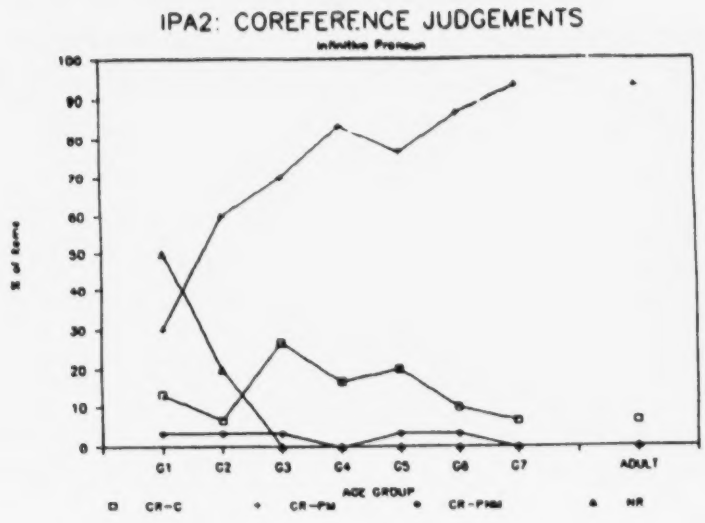

Figure 5 Infinitive Pronoun

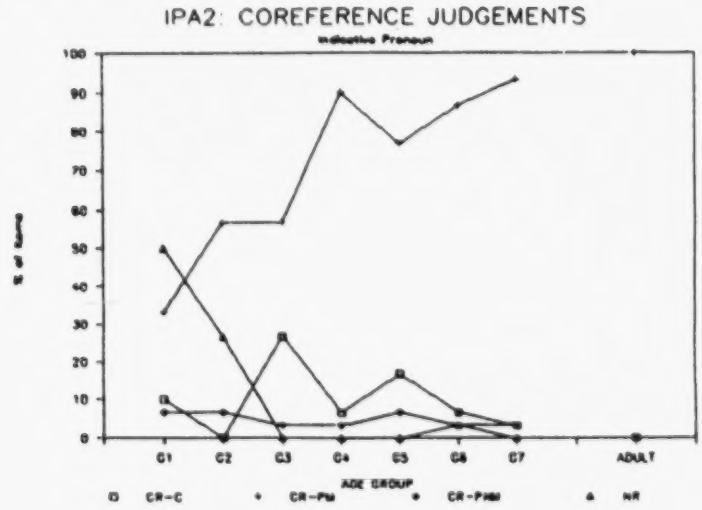

Figure 6 Indicative Pronoun

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children do quite well on pronouns. Thus, they consistently choose the long-distance antecedent for the pronoun. Very few children allow the pronoun to refer to the extra-clausal referent, that is, to the doll which was present in the experimental setting but not mentioned in the test sentence. But this may be due to the fact that it is pragmaticaly more felicitous for a pronoun to refer back to a mentioned doll. This same factor may be responsible for the results obtained in the infinitive sentences, given in figure (5), where the children and the adults prefer the doll mentioned rather than the outside NP. Recall that adult judgments on infinitive sentences of this type usually indicate a strong preference for extra-clausal antecedent. Thus, to sum up the results of the pronoun sentences, Icelandic children have knowledge of principle B of the binding theory relatively early in their linguistic development, and they show a steady increase in performance as a function of age, reaching 90% correct by age 6;0.

Chien & Wexler (1987a), based on their study of English speaking children, propose that there is a developmental lag in the acquisition of pronouns relative to anaphors. While the Icelandic children do exceedingly well on pronouns, as do the Chinese speaking children in Chien & Wexler (1987b), our preliminary results suggest that in Icelandic as well, correct usage of pronouns may lag slightly behind correct performance with sig. These results will be discussed further in Hyams & Sigurjónsdóttir (in preparation).

In conclusion, the experimental results presented here shed light not only on the developmental question of how children determine the binding properties of referentially dependent elements, but has also led to a reformulation of a widely accepted analysis of the adult grammatical system. Hence, we hope to have shown that acquisition results can inform the theory of grammar.

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Linguistic Representations of Children's *Wh*-Questions[1]

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Abstract. I determined when 12 children acquired different types of questions. I found that they began to ask object questions before they began to ask subject questions. I also found that they acquired argument questions before adjunct questions. These two results could be the reflection of the syntactic structure of the questions or the learning mechanism of the children. It is possible that the children mastered object questions before subject or adjunct questions because object traces are theta-governed whereas adjunct and subject traces are antecedent-governed. Another possible explanation is that they acquired object questions first because object gaps are more obvious than subject or adjunct gaps. While the data are not conclusive, they are more consistent with the former explanation.

1. Do children's subject questions have gaps?

Introduction

A subject question asks a question about the identity of the subject of a sentence while an object question asks a question about the object of a sentence. One way subject and object questions differ is that an object question clearly has a gap while a subject question may or may not have a gap. Consider for a moment questions that have the verb *meet*. The verb *meet* requires a subject and an object. Therefore, the only viable structure for the object question *who will Mary meet* is one in which there is a gap following the verb. This gap is somehow linked to the sentence-initial *who* (*who*$_i$ *will Mary meet* t_i*.?*).

Now compare the subject question *who will meet John?* with the sentence *Mary will meet John*. There are two possible structures for this question. One possibility is that there is no gap and the *who* is in the same place as *Mary* (*who will meet John?*). Alternatively, there could be an invisible gap between *who* and *will* (*who*$_i$ t_i *will meet John?*). Which structure is correct? According to Government-Binding (GB) theory (Chomsky, 1986), both subject and object questions have gaps. According to Generalized Phrase Structure Grammar (Gazdar, 1981; Gazdar *et. al.*, 1985), subject questions do not have gaps.

Gazdar (1981) predicts that, because subject questions are gapless, they will be acquired before object questions. He writes (1981, p. 172):

> A derived category [a gap] has to contain twice as much syntactic information as a basic category. This suggests that structures which involve derived categories will impose a heavier processing load than those that do not. If this is so, then our analysis predicts that matrix subject relatives and questions will be significantly easier to process than all other relatives and questions. This prediction is borne out, both developmentally and for adult speakers by recent psycholinguistic work.

All of the developmental research (e.g., Ervin-Tripp,1970; Cairns and Hsu, 1978; Tyack and Ingram, 1977; and Stewart and Sinclair, 1975) examines children's **comprehension** of subject and object questions. Contrary to what Gazdar suggests, this research does not uniformly show that children comprehend subject questions earlier or

with greater ease than object questions.[2] In addition, comprehension research may not be the most appropriate means to determine whether subject questions have gaps. Research by Wanner and Maratsos (1978) and Frazier, Clifton, and Randall (1983) indicates that the greater the distance between a word and its gap, the more difficulty adults have processing the sentence. Therefore, even if subject questions **do** have gaps, children might comprehend them more readily than object questions because the distance between the *wh*-word and its gap would be less for subject questions than for object questions.

If Gazdar is right and subject questions are gapless, then (all else being equal) subject questions should be acquired earlier and used with greater ease than object questions. In Analysis 1, I determined when 12 children first produced subject and object questions. In Analysis 2, I examined how often they produced subject and object question. In Analysis 3, I determined whether the order of acquisition of subject and object questions was merely a reflection of adult input frequencies. In Analysis 4, I attempted to determine whether the order of acquisition was actually just an artifact of sampling.

Analysis 1. Age of acquisition of subject and object questions

I examined the spontaneous speech transcripts of the 12 children listed in Figure 1 and determined when each child asked her first subject and object *who*, *what*, and *which* questions.[3]

Figure 1: Children included in the transcript analyses

Corpus collected by	Child	Ages	# lines w/ *wh*-words
Brown (1973):	Adam	2;3-5;2	4,859
	Sarah	2;3-5;1	2,350
	Eve	1;6-2;3	651
MacWhinney (n.d):	Ross	2;10-6;6	2,372
	Mark	1;5-4;7	830
Snow (1983):	Nathan	2;6-3;9	1,724
Sachs (1983):	Naomi	1;2-4;9	1,023
Bloom (1973):	Peter	1;10-3;2	1,920
	Allison	1;4-2;10	37
Higginson (1985):	April	1;10-2;11	263
	May	0;11-0;11	0
	June	1;3-1;9	37
Total number of lines with *wh*-words:			16,066

Results. Overall, the children asked their first object question 1.5 months **before** they asked their first subject question. This difference was significant by sign test ($p < .02$) and marginally significant by t-test (t (9 d.f.) = -2.04, $p < .07$).[4] On average, they asked their first *who* subject question 0.5 months before they asked their first *who* object question. This difference was not significant by either sign ($p > .34$) or t-tests ($p > .26$). On average, they asked their first *what* object question 2.5 months before they asked their first *what* subject question. This difference was marginally significant by sign test ($p < .06$) and t-test (t (9 d.f.) = -2.04, $p < .07$). Lastly, they asked their first *which* object question a mean of 8.8 months before they asked their first *which* subject question. This

difference was marginally significant by sign test ($p < .06$) and significant by t-test ($t(3$ d.f.$) = -3.51$, $p < .04$).

Analysis 2. Relative frequency of subject and object questions

I determined the frequency of subject and object *who*, *what*, and *which* questions in the transcribed speech of each of the 12 children shown in Figure 1.

Results. Overall, the children produced 3 times as many object questions as subject questions. Thirty-three percent of their *who* questions , 88% of their *what* questions, and 77% of their *which* questions were object questions.

Analysis 3. Adult input of subject and object questions

Perhaps the children acquired object questions before subject questions because the people who spoke to them asked more object questions than subject questions. For each of the 12 children, I determined how frequently the adults speaking to each child asked subject and object *who* and *what* questions. I determined the percent of adult *who* and *what* questions that were subject questions [5]

Results. I found no significant correlation between the percent of adult and child *who* questions which were subject questions ($r=-.09$). I also found no significant correlation between the percent of adult and child *what* questions which were subject questions ($r=.26$). In addition, the adult percentages did not correlate significantly with the ages at which the children acquired subject or object *who* or *what* questions.(all correlation coefficients were between -.52 and +.39).

Analysis 4. Sampling artifacts and subject and object questions

Perhaps the children only appeared to acquire object questions before subject questions because of sampling. If they asked many more object questions than subject questions, then a random sample of their speech would be more likely to include object questions than subject questions. If this is why object questions appeared first in the transcripts, then we would expect that the types of questions that appeared frequently in the transcripts would be "acquired" earlier and questions that were infrequent would be "acquired" late. I determined whether the children's frequencies and ages of acquisition were correlated in this manner.

Results. This pattern of results was not obtained. Frequent use of subject *who* questions was fairly correlated with early acquisition of subject *who* questions ($r = -.81$). However, frequent use of object *who* questions correlated with **late** acquisition of *who* object questions ($r = .74$). The same pattern held for *what* questions, though the correlations were not even marginally significant. ($r = -.40$ for subject *what* questions and $r= .31$ for object *what* questions).

Discussion

Gazdar (1981) predicts that subject questions should be easier to learn and use than object questions because subject questions are gapless. However, the production data discussed in Analyses 1 and 2 suggest that children find subject questions **harder** than object questions. Thus, the results of Analyses 1 and 2 suggest that Gazdar is wrong on at least one of two points. Either gaps don't make a structure harder to acquire and use, or subject questions have gaps.

It is somewhat surprising that the children used object questions earlier and more frequently than subject questions. There are at least 5 reasons why we might expect subject questions to be easier than object questions First, the distance between the *wh*-word and

the gap is shorter in subject questions than object questions. Second, subject questions generally have fewer words than object questions. Third, subject questions don't have overt subject-auxiliary inversion. Fourth, subject questions never require *do*-support. Fifth, subject questions are homologous in structure to simple declaratives.

The children might have acquired object questions before subject questions because they heard more object questions than subject questions. The results of Analysis 3 suggest that this is not the correct explanation. Another possible explanation is that object questions only appear to have been acquired before subject questions because of speech sampling. While it is not possible to rule out this explanation, the results of Analysis 4 suggest it is not correct.

2. Do children's argument and adjunct questions differ structurally?

Introduction

Argument questions ask questions about an argument of a sentence. Argument questions include all *who* and *what* questions and certain *where* questions (e.g., *where did Mary put the book?*, *where do the books go?*, *where is the book?*, etc.) and *how* questions (e.g., *how big is the book?*, *how many did she have?*, etc.). Adjunct questions ask questions about an adjunct in a sentence. Adjunct questions include all *why*, *when*, and *how come* questions, and most *where* and *how* questions (e.g., *where did Mary meet John?* and *how did Mary know John?*).

Is there any developmental evidence to suggest that argument and adjunct questions differ structurally? Many researchers (e.g, Brown, 1973; Ervin-Tripp, 1970; Tyack and Ingram, 1977; Labov and Labov, 1978) have reported that children usually begin to ask *who* and *what* questions (i.e., argument questions) before they ask *why*, *when*, and *how* questions (i.e., adjunct questions). However, no one has done a longitudinal study designed to test whether argument questions are acquired before adjunct questions. The following is such a study. In Analysis 1, I compared the average age of acquisition of argument and adjunct questions. Analysis 2 is an attempt to tease apart the conceptual and syntactic factors which may influence when argument and adjunct questions are acquired. In it, I compared when argument and adjunct *where* and *how* questions were acquired. In Analysis 3, I attempted to determine whether children's difficulty with adjunct questions reflects a general difficulty with **adjuncts** or a specific difficulty with adjunct **questions**. I did this by comparing the acquisition of locative questions and declaratives.

Analysis 5. The mean age of acquisition for argument & adjunct questions

I recorded the age at which each of the 12 children began producing nonroutine, sentential examples of each type of question. For each child, I calculated the mean age of acquisition for argument questions and adjunct questions. For argument questions, I did this by averaging together the age of acquisition for *who*, *what*, *which*, argument *where*, and argument *how* questions. For adjunct questions, I averaged together the age of acquisition for *why*, *when*, *how come*, adjunct *where*, and adjunct *how* questions. I then compared the average age of acquisition for argument and adjunct questions.

Results. Averaging across *wh*-words and children, the children asked their first argument question 7.1 months before they asked their first adjunct question. This difference was statistically significant by both the sign ($p < .001$) and t-tests ($t(9) = 6.10$, $p < .0001$).

Analysis 6. Syntactic versus conceptual differences and difficulties

The children might have found adjunct questions more difficult than argument questions for conceptual reasons or structural reasons. One way to determine whether syntactic or conceptual complexity delayed the acquisition of adjunct questions is to compare when they acquired argument and adjunct *where* questions. Argument and adjunct *where* questions are both questions about locations. Adjunct locations aren't any more conceptually complicated than argument locations. Therefore, any difference in age of acquisition between adjunct and argument *where* questions cannot be due to differences in conceptual complexity. The same logic holds for argument and adjunct *how* questions. Any difference in the age of acquisition for argument and adjunct *how* questions cannot be attributed to differences in conceptual complexity. I compared when each of the 12 children acquired argument and adjunct *where* and *how* questions.

Results. On average, the children asked their first argument *where* question 6.8 months before they asked their first adjunct *where* question. This difference was significant by sign test ($p < .002$) and t-test ($t(8)= 3.49$, $p < .008$). On average, they asked their first argument *how* question 7.5 months before their first adjunct *how* question. This difference was also significant by sign test ($p < .031$) and t-test ($t(5) = 3.45$, $p < .018$). The difference for *how* questions is particularly striking for two reasons. First, most of the children's early *how* argument questions had *how* phrases (e.g., *how big is the book?*) while most their early *how* adjunct questions just had *how* (e.g., *how does she swim?*). Second, most of their early argument *how* questions were quantificational queries, something which is conceptually quite sophisticated.

Analysis 7. Acquisition of argument and adjunct questions and declaratives

Perhaps the children acquired adjunct questions late because of a general problem with adjuncts rather than because of a specific problem with adjunct questions. These two possibilities can be teased apart by comparing when they acquired locative arguments and adjuncts in declaratives with when they acquired *where* argument and adjunct questions. If they acquired *where* adjunct questions late because they acquired declarative adjuncts late, then we would expect to find a significant difference in the age of acquisition for argument and adjunct locative declaratives just as we found a significant difference for *where* questions. We would also expect that the children would have acquired adjunct questions soon after they acquired adjunct declaratives. I determined when each of the 12 children acquired locative arguments and adjuncts in declaratives by searching for all of the children's lines which had the prepositions *in*, *on*, or *at*.[6]

Results. The children began using declarative argument locatives an average of 0.5 months before they began using declarative adjunct locatives. This difference was not significant by either sign ($p<.50$) or t-tests ($t(9) = 0.75$, $p > .475$). In contrast, they acquired *where* argument questions 6.8 months before *where* adjunct questions, a difference which was significant by both sign test ($p < .002$) and t-test ($t(8)= 3.49$, $p < .008$). On average, *where* argument questions appeared 3.5 months after declarative argument locatives and *where* adjunct questions appeared 10.7 months after declarative adjunct locatives.

Discussion

The results of Analysis 5 indicate that the children acquired argument questions at a significantly earlier age than they acquired adjunct questions. The results of Analysis 6 suggest that this difference was not due to differences in the conceptual complexity of argument and adjunct questions. The results of Analysis 7 suggest that the difficulty posed

by adjunct questions is a specific problem with adjunct questions and not a general problem with adjuncts.

3. Why do children acquire questions in this order?

Introduction

The results of the first set of analyses suggest that object questions are acquired before subject questions and the results of the second set of analyses suggest that argument questions are acquired before adjunct questions. What accounts for these two results? Are they both due to a single factor? Two different sorts of factors could account for these results. They could be a reflection of either the syntactic structure of the questions or the learning mechanism of the children.

Structural differences between the different types of questions could cause children to acquire object and argument questions before subject or adjunct questions. According to current GB theory (Chomsky, 1986), object questions differ from subject and adjunct questions in the way the *wh*-trace is governed. GB's Empty Category Principle (ECP) states that all traces must be properly governed. There are, however, two ways a trace can be properly governed. A *wh*-trace can be directly theta-governed by a verb. This is how object *wh*-traces are governed (the simplified structure is shown in i.below). Alternatively, a *wh*-trace can be indirectly or antecedent-governed by a *wh*-word via a chain. This is how subject *wh*-traces and adjunct *wh*-traces are governed (simplified structures are shown in ii. and iii. below).

i.	object	Who$_i$ will [$_{IP}$ Mary [$_{VP}$ meet t $_i$?]]]
ii.	subject	Who$_i$ [$_{IP}$ t$_i$ [$_{VP}$ meet John?]]]
iii.	adjunct	When$_i$ will [$_{IP}$ Mary [$_{VP}$ meet John t$_i$?]]]

Theta-government by a verb is more direct and less complicated than antecedent-government by a *wh*-word. (For example, *wh*-words that are directly theta-governed can be extracted more freely than *wh*-words that are antecedent-governed.) It seems plausible, therefore, that questions with theta-governed gaps (i.e., object questions) would be acquired before questions with antecedent-governed gaps (i.e., subject and adjunct questions). I will call this first theory the Government Theory.

Alternatively, children may have some kind of learning procedure which permits them to acquire object questions before subject and adjunct questions. For example, children might ask argument questions first because they notice object gaps more than subject or adjunct gaps. I will call this theory the Visibility Theory.

Let's start with the assumption that children know the argument requirements of verbs. They know, for example, that the verb meet requires that there be someone who meets and someone who is met. When they hear the adjunct question *why did Mary meet John [gap]?*, they can ignore the sentence-initial *why* and its gap and still have a grammatical string (*did Mary meet John?*). When they hear the subject question *who met John?*, they must treat *who* as the subject of the sentence or the string will be ungrammatical (**met John*). They do not, however, have to treat *who* as a question word with a gap. They could treat *who* as some kind of pronoun or proper name.

Object gaps are quite obvious. Upon hearing the object question *who did Mary meet [gap]?*, children might notice that the verb *meet* isn't followed by an object. They might be compelled to search for a suitable object. In their search, they might notice the

who at the front of the sentence and make the connection between the missing object at the end of the sentence and the *who* at the beginning of the sentence. Notice that object gaps are only this blatant in questions with verbs that **require** an object. When children hear object questions with verbs that optionally take objects (e.g., *what did she eat?*), they can assume that these are merely sentences in which the verbs don't take an object. They can ignore the *wh*-word and its trace and still have a grammatical string (*did she eat?*).

Analysis 8. Adult obligatory object questions and child acquisition

The Visibility Theory suggests that parents who ask mostly obligatory object questions will have children who acquire object questions earlier than parents who rarely ask obligatory object questions. The Government Theory does not make this prediction. For each of the 12 children, I determined the percentage of the adults' *who* and *what* object questions that had obligatory objects.

Results. Overall, 98% of adult *who* object questions and 91% of adult *what* object questions had obligatory objects. The percentage of adult obligatory object questions did not correlate with the age of acquisition of either subject or object *who* or *what* questions (all correlation coefficients were between -.13 and +.31).

Summary

In this paper, I determined when 12 children began to ask different types of questions. I found that they began to ask object questions before they began to ask subject questions (Analyses 1-4) and argument questions before adjunct questions (Analyses 5-7). These results could be a reflection of the relative visibility of object traces and adjunct and subject traces (the Visibility Theory). Alternatively, they could be due to differences in how object traces and adjunct and subject traces are governed (the Government Theory). The results of Analysis 8 are more consistent with the Government Theory than the Visibility Theory. In conclusion, the results of all of the transcript analyses are consistent with the hypothesis that children are able to theta-govern traces before they are able to antecedent-govern traces.

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[2] Children understood object questions that had *do*-support as well, or better than, they understood subject questions. The only object question that they didn't comprehend as well as subject questions were object questions that had progressive participles (e.g., *what is she kicking*).

[3] I did not include the following 6 types of questions in the subject/object analyses:

1. nonsensential questions (e.g., *what?*)
2. routine questions (e.g., *what's that?*)
3. questions that could be subject or object questions (e.g., *what is a funny shape?*)
4. questions that could be subject or sentential questions (e.g., *what is wrong with Billy?*)
5. questions that questioned a verb (e.g., *what is he doing?*)
6. "*what for*" questions (e.g., *what did he do that for?*).

[4] All significance levels are for two-tailed tests.

[5] The adult percentages in Analyses 3 and 8 are the percentages for adult questions that were asked prior to the age at which the children acquired the type of question under investigation.

[6] I chose to search for these 3 prepositions because they occur most frequently in the transcripts. It is possible that by using this restricted search I overestimated the age at which the children acquired locative

arguments and adjuncts in declaratives. It seems likely, however, that this search inflates the acquisition age equally for arguments and adjunct locatives.

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WORD LEARNING STRATEGIES IN TWO-YEAR-OLD CHILDREN: EVIDENCE FOR CATEGORY HIERARCHIES

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Ten years ago Susan Carey (1978) described young children as word learning wizards. Her claim was based on research indicating that children rapidly accomplish the complex task of learning their first language with not much in the way of explicit coaching. The problem of accounting for children's ability to master language is exacerbated by the existence of an indefinite number of possible meanings for any word that is defined by ostension (Quine, 1960). Thus, we assume that children must make use of heuristics or strategies to simplify the task of figuring out what a new word means. Here, we have examined in three experiments, how the use of strategies might help children acquire the language to express category hierarchies.

In our first experiment, we investigated the possibility that children use their current linguistic knowledge to constrain the potential meanings of new words. This idea has been proposed by Clark (1983, 1987) in her Lexical Contrast Theory and by Markman in her work on Mutual Exclusivity (Markman, 1987; Markman & Wachtel, 1988). One implication of this idea is that how children interpret a new word should depend in part on whether they already know a word for what is being named. This is the hypothesis we tested in Experiment 1 by comparing children's interpretations of new words given to objects they could already name (e.g., dogs) with their interpretations of new words given to novel objects that were created to be unlike any kind the children might know.

The subjects in this and our other experiments were about two years old (mean age = 2-2), and thus were in the early stages of language acquisition. In all three studies we used the same procedure for teaching children a new word and assessing their interpretation of it. The child was brought into a room where there were four toys. In a brief play session, one of the toys was named six times by the experimenter (e.g., "This is a fep."). To test how children interpreted the new word, we asked the children to perform a series of actions (e.g., "Can you throw a fep in the air?"). There were at least five trials in which children were asked to do something with a [novel word]. On other filler trials, children were just handed a toy for performing the actions. We included these latter trials so that the testing period would not seem to focus exclusively on the named toy. By looking at the toys that children selected when asked to do things with, for example, a "fep", we could get some information about how children had interpreted the new word.

Experiment 1 was designed to determine if children interpret a new word for an object differently, depending on whether they already know a name for the object. Children in the unfamiliar condition played with two kinds of unfamiliar stuffed animals. Two of the toys were shaped somewhat like whales, but they had long green tails, round ears, and large eyes. One of these was made from pale green fake fur and one was made from yellow and black plaid material. The other two toys had roughly triangular shapes. These stuffed animals had white hair, red noses and feet, and smiling faces. The experimenter named one of these toys six times (e.g., "This is a fep."). These toys were novel; thus, our expectation was that the children would interpret the new word as a name for the category that included the named object (category being defined by overall shape and parts). A category interpretation of the new word would lead children to pick between the two objects from the named category across the test trials, when children were asked to perform actions with a "fep."

In a second condition, children played with toys that they already knew the names of: two stuffed dogs and two stuffed birds. One of the dogs and one of the birds were made out of pale green fake fur; the other two toys were made out of bright yellow and black plaid material. If children assume that a new word cannot be a synonym for a known word, how will they interpret a new word given to a dog or bird? One possibility is that children might interpret the new word as referring to some property of the object. Using a different paradigm with a different naming context, Markman and Wachtel (1988) found some support for this idea. We were able to assess this possibility because the stimuli in our experiment were designed to allow detection of a property interpretation for the new word, as well as a category interpretation. If "fep" referred to some property of the named object, children should pick the named object and the object made from the same material across the test trials. Our main hypothesis, however, was simply that we would find a difference in how children interpreted the new word as a function of familiarity with the toys.

Table 1 shows the mean proportion of object selections as a function of familiarity of the toys. It is clear from this table that although children in the unfamiliar condition showed a preference for picking the named object, they picked the other object from the named category a significant proportion of the time (.30). This pattern held both within and across subjects - that is, individual children tended to pick both the named object and the other member of the category. This result was consistent with an interpretation of the new word as referring to the category of the named object. Children in the familiar condition, however, picked the named object on almost all the test trials (.84). There was no evidence that they had interpreted the new word as referring to a property of the named object. The differences between the familiar and unfamiliar conditions in the proportions of named

object choices and the proportions of choices of the other object from the named category were significant ($p < .05$). These results support the notion that the way a child interprets a new word depends in part on whether the child already knows a label for the named object. This effect of familiarity was replicated in a second experiment using different stimuli (Taylor & Gelman, 1988).

The results of Experiment 1 indicate that when children learn a new name for an object that already has a known name (e.g., a "fep" for a dog), the new word is used to refer only to the specific object that was named. This result provides evidence that young children set up a contrast between the old and new word, but does not provide any information about how children interpreted the new word. Children's tendency in the familiar condition to use the new word to refer only to the object that was explicitly named could be explained in at least four different ways:

(1) Children interpreted the new word as a proper name for the object. Past research would argue against this possibility because the form class (e.g., a fep) was inconsistent with a proper name interpretation. Even by two years of age, children know that words for categories, but not words for individuals, take an article (Gelman & Taylor, 1984; Katz, Baker, & Macnamara, 1974).

(2) Children narrowed the extension of the familiar category and interpreted the new word as a name for a new contrasting category (e.g., an object that the child initially thought was a dog is now thought to be a fep; furthermore, dogs and feps are nonoverlapping categories). This interpretation is consistent with how Clark (1983) has proposed children eventually narrow down their overextensions (e.g., how children learn to call what they once called "horse" by the name of "zebra").

(3) Children interpreted the new word as referring to a subordinate level category (e.g., a kind of dog). The results of research demonstrating children's difficulty with classification hierarchies (for a review, see Markman & Callanan, 1984) would argue against this possibility.

(4) Children were confused when they heard the new name (a "fep" to refer to a dog) and adopted the conservative strategy of using the new word only in a way they were certain was correct (i.e., to refer to the object that had been explicitly named).

This list is not exhaustive. There is at least one other possibility, namely, that children interpreted the new word as referring to a category that overlapped with the known category. For example, it could be that some but not all feps are dogs and some but not all dogs are feps. The possibility of an overlap interpretation could not be assessed with our present word learning paradigm; however, we were able to assess the four possibilities listed above with Experiments 2 and 3. Experiment 2 provided a way to distinguish among possibilities (1), (2-3), and (4). Experiment 3 provided a way to distinguish possibilities (2) and (3).

In Experiment 2, half the children were taught a new word for a ball and half were taught a new word for a toy dog. By two years of age or younger, children appreciate that some kinds of objects (e.g., dogs) typically get their own special names and some kinds of objects (e.g., balls) do not (Gelman & Taylor, 1984; Katz, Baker, & Macnamara, 1974). Young children do not interpret a word given to a toy like a ball as a name for that particular object, even when the experimenter uses a proper noun construction ("this is Wug"). Thus, if children in this experiment use a proper name interpretation of the new word (explanation # 1), they should adopt this interpretation only when it is semantically appropriate, that is, only when learning a new name for a dog. When an object such as a ball is named, a proper name interpretation should be blocked.

If picking the named toy reflects a subordinate category interpretation (explanation # 3), children's use of the new word should be affected by the degree of similarity among the category exemplars. When two exemplars are similar enough to share subordinate category membership (e.g., two wire-haired terriers that differ only in their sweaters and ribbons), a new word given to one exemplar should be considered appropriate for the other as well. When the exemplars are quite different (e.g., a wire-haired terrier and a basset hound), children should use the new word to refer to the named object only. By varying the similarity of the exemplars of each category, it was possible to test this prediction. A main effect for similarity of category exemplars would also be expected if children interpreted the new word as referring to a category that contrasted with the known category at the same level (explanation # 2).

If children picked the named toy because they were confused about the meaning of the new word (explanation #4), neither type of toy (dog or ball) nor the similarity of category members (similar or different) should affect their performance. In all four conditions, children would be expected to use the new word to refer only to the object that had been named by the experimenter.

32 two-year-old children were randomly assigned to a dissimilar exemplars condition or a similar exemplars condition and learned a new word for a dog or a ball. In the dissimilar exemplars condition, the toys were a wire-haired terrier, a basset hound, a beach ball and a soccer ball. In the similar exemplars condition, the toys were two terriers distinguished by their sweaters and two beach balls that were colored differently. Each child learned a new name for one of the toys and was tested for his or her interpretation of the new name as in Experiment 1. The results of this experiment were consistent with both the subordinate category and contrasting basic category interpretations (2 & 3) for the new word. There was a significant main effect for the similarity of category exemplars, $p < .01$. When the exemplars were similar, children tended to choose both the named object and the object from the same category when asked to do things with a

[novel word]. When the exemplars were dissimilar, children tended to choose only the named object (see Table 2).

According to the results of Experiment 2, children interpret a new word given to a dog as a subordinate category like terrier or as a contrasting basic category like wolf. To distinguish these two possibilities, we tested eight additional two-year-old children, using the toys that had been used in the Dissimilar Exemplars condition of Experiment 2. The purpose of Experiment 3 was to determine whether children who learned a new word for a familiar object would still consider the object to be an appropriate referent of the old word. The naming portion of this experiment was identical to that of Experiment 2. However, in the testing portion children were tested on the conventional label for the named object ("dog" or "ball") rather than on the novel word they had just learned. Then, at the end of the session, the experimenter asked the child to point to a [novel word]. If children interpreted the new name as a subordinate category label, then the old name (e.g., "dog") still applies to the named object. However, if the new name was interpreted as referring to a previously unknown contrasting category, the old name should no longer be considered correct.

All eight children in this experiment selected both members of the named category when asked to perform the series of actions with a dog or ball, as shown in Table 2. Thus, learning a new word did not induce a restructuring in children's understanding of the already known category label. When asked to point to a [novel word] at the end of the session, five children correctly picked the named toy (two children who had been taught a new name for a ball and three who had been taught a new name for a dog). The other three children gave no response. A binomial test on these data was significant, $p < .05$, indicating that picking the named toy occurred more often than predicted by chance. Thus, most of the children still remembered the new label at the end of the procedure. Taken together, the results of Experiments 2 and 3 suggest that children readily interpret a new name for a familiar object as a subordinate category label, without revising their interpretation of the familiar word.

Conclusions

With these three experiments we have demonstrated that two-year-old children interpret a new word differently depending upon whether they already know a word for the object being named. In addition, our results are consistent with the idea that two-year-old children tend to interpret a new noun given to a familiar object as a subordinate category term like terrier or collie. This finding is somewhat surprising, given that older children in other contexts often demonstrate difficulty with hierarchical relations (see R. Gelman & Baillargeon, 1983; Markman & Callanan, 1984, for reviews).

We believe that two-year-old children were able to construct a simple hierarchy in our studies because the context of our procedure was particularly supportive of a subordinate category interpretation for the new words the children learned. In particular, the basic level categories were ones that were likely to have been well established (e.g., dogs and balls) and the subordinate level distinctions were perceptually very clear. In contrast, Merriman's (1986) failure to find subordinate category interpretations for new words may have been related to the fact that his subjects were required to learn names for both levels of the hierarchy in the same session. In addition, when the distinction between different subordinate level categories is perceptually subtle (e.g., the placement of two protrusions and a small shape on an abstract geometric form), children have considerable difficulty in learning the categories (Mervis & Crisafi, 1982).

The ability of our subjects to construct a hierarchy may also have been helped by the fact that the task in these experiments was fairly simple. Children were not required to display understanding of the asymmetry of inclusion hierarchies, as in the Piagetian class-inclusion problem (Inhelder & Piaget, 1964). Children were not asked to sort objects into groups, which is a task that requires children to hold in mind simultaneously at least two categories and to apply a criterion both consistently and exhaustively, while often imposing additional information processing demands as well (Markman & Callanan, 1984). Rather, children were simply asked to identify at least one instance of each category in question. An important corollary to this point is that we certainly do not claim that children appreciate the asymmetry of hierarchical relations, nor even that they can necessarily keep in mind both levels of a hierarchy at once. Rather, we suggest that children are learning the language of hierarchies in an accurate manner, and so can represent more than one hierarchical level by means of language.

These results have direct implications for recent theories concerning children's strategies in acquiring new words. We found evidence for lexical contrast in young two-year-old children; when children heard a new word given to an object with a familiar label, they did not treat the new and old words as synonyms. Thus, children's interpretation of the new word was partly constrained by their knowledge of a familiar word for the same object. However, children did not assume mutual exclusivity; inclusion relations specifically violate mutual exclusivity because the higher and lower level terms refer to partly overlapping sets. Taken in conjunction with other recent findings on early word learning, it appears that children are capable of appreciating a wide range of semantic relations from a very young age, including hierarchical inclusion (this work), mutually exclusive contrast (Markman, 1987; Markman & Wachtel, 1988), and overlap (Merriman, 1986).

Table 1

Mean proportion of object selections as a function of familiarity (Experiment 1)

	Named toy	Same category	Same material	Other
Familiar (n = 8)	.84	.09	.06	.01
Unfamiliar (n = 8)	.59	.30	.06	.05

Table 2

Mean proportion of object selections as a function of similarity

	Named Toy	Other toy from same category	Other
EXPERIMENT 2: Novel name			
Similar exemplars	.68	.32	.00
Dissimilar exemplars	.96	.04	.00
EXPERIMENT 3: Conventional name			
Dissimilar exemplars	.47	.53	.00

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ICONICITY IN SEMANTICS: A CASE OF SUPRASEGMENTAL MARKING IN THE ACQUISITION OF THE ENGLISH PLURAL

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In earlier papers, we presented case studies of language-impaired children who had adopted suprasegmental strategies for marking the plural (cf. Camarata & Gandour, 1985; Camarata & Erwin, in press). Because these children were suffering from impaired phonological systems, they could not produce the usual segmental form of the marker (e.g., [s, z, əz]). We argued that the suprasegmental strategy of increasing duration, intensity, and frequency was highly idiosyncratic, and to be candid, we felt it unlikely that such cases would appear elsewhere. Additionally, these case studies were offered in support of those theories of language acquisition which view the child as an active learner as they gain linguistic competence (Kiparsky & Menn, 1977; Macken & Ferguson, 1983).

However, if the parameters surrounding such suprasegmental marking are examined in more detail, it is evident that many normal children face precisely the same kinds of phonological constraints found within these earlier case studies. For example, fricatives are acquired rather late, and final clusters are acquired very late (Ingram, 1981). However, crosslinguistic studies demonstrate the importance of the plural because it is acquired early universally (Slobin, 1970). Thus, the constraints for suprasegmental marking of plural in language impaired children should be problematic for language normal children as well because they also would have the early motivation for signaling plural in the absence of a developed phonological system.

The purpose of this paper is to present yet another case study involving suprasegmental marking of plural that differs from the previous papers in one important aspect: the child described herein is not language impaired. Rather, she is progressing normally. In addition to presenting the specific acoustic-phonetic structure of this suprasegmental marking, it will be argued that such marking is perhaps a rather common phase in the acquisition process resulting from the iconic aspects of child language acquisition (cf. Slobin, 1985).

Case Profile

The subject of this investigation, KM, was 2:7 (years:months) at the time of data collection. She is from a monolingual English speaking home and achieved developmental milestones within the normal range according to parental report. The results of the Denver Developmental Scale verified these reports as KM was within normal limits on both the cognitive and motor portions of this assessment instrument.

Morphological Analysis

In terms of linguistic development, results of standard language sampling procedures and scoring derived from Brown's (1973) procedures indicated that she acquiring the language in a typical fashion as well. The Mean Length of Utterance (MLU) from a 150 utterance language sample was 2.2; well within the normal range presented in Miller's (1981) normative data. This MLU placed KM in Brown's stage II, when present progressive, on, and plural often appear in a child's speech. Both present progressive and on were used within the sample; indeed these were the only grammatical morphemes evident within her linguistic system. However, the adult version of the plural was absent from the sample.

Phonological Analysis

A phonological analysis suggested that a lack of development within the segmental domain was a likely source for the difficulty in plural production. Like many children, KM could not produce final fricatives with any degree of consistency and no final clusters were evident within her speech. To be sure, KM's phonological system was much richer than those of the children described in the earlier reports (Camarata & Gandour, 1985; Camarata & Erwin, in press) as she produced stops with a high degree of accuracy and was in the process of learning the fricatives. However, like these earlier cases, she did have full control of the segments needed to produce plural. Thus, the basic convergence of phonological and morphological factors resulting in suprasegmental marking in the earlier reports were evident here as well.

Method

The methodology developed by Camarata and Gandour (1985) for the elicitation and analysis of children's plural productions were adopted within the present investigation. In addition, the elicitations and subsequent acoustic analyses were performed by a research assistant who was blind to the purpose and theoretical underpinnings of the study. The language sampling session and subsequent word elicitation session were completed within a quiet room in

KM's home and audio recorded in stereo using a Marantz PMD 420 cassette recorder and two Realistic 33-1056A electret microphones attached to KM's shirt approximately 10cm below her mouth.

Word Elicitation

The procedures employed by Camarata and Gandour (1985) were once again used here. Because it was important to avoid specific cuing of the singular-plural distinction, a naming task was utilized in order to ensure representative productions. KM's original language sample and diary information provided by her parents were examined to determine which nouns she had acquired. This list of nouns was further analyzed to determine which included only single syllable word structures.

A randomized list of singular and plural versions of these words was then constructed such that no singular and plural forms of the same word appeared contiguously. Pictures of these singular and plural nouns were then presented in the following manner. The research assistant presented the picture without any verbal prompt whatsoever. Often, KM would spontaneously name the picture following this presentation. If she did not produce the word spontaneously, the research assistant asked her to: "Name the picture." This verbal prompt was used for both singular and plural versions of the targets in order to avoid experimenter bias intruding upon the child's productions. If KM did not produce the word following this prompt, the investigator went on to the next picture. At not time was the target word produced by the research assistant and thus, no imitative productions were included in the acoustic analyses. A total of 13 singular/plural word pairs were obtained within the elicitation session.

Acoustic Analyses

A Kay 6095/6097 visipitch-Apple IIe interface coupled to a Sony TC-FX220 cassette deck was used for the acoustic analyses. The acoustic parameters of interest included duration; peak, mean, and delta fundamental frequency (F_0); and peak, mean and delta intensity. Duration was measured using the procedures suggested by Peterson and Lehiste (1960) by determining elapsed time from the onset to the offset of the vowel nucleus. Initial and final consonants were not included as the voice onset of these can vary widely at this age (cf. Leonard et al. 1985). The F_0 and intensity measures were completed over the duration described above. The peak and mean values were simply derived from the online video display provided by the visipitch and entered directly into the computer. The delta values were calculated by subtracting the onset F_0 (or intensity) from the offset F_0 (or intensity). The standard

error of measure for these procedures were as follows: 2.1 msec for duration; 1.0 Hz, 1.1 Hz, and 2.3 Hz for peak, mean, and delta F_0, respectively; and 0.5 dB, 0.6 dB, and 2.7 dB for peak, mean, and delta intensity respectively.

Results

The results of the acoustic analyses are presented in Table 1. The means and standard deviations of the selected acoustic parameters are greater consistently in the plural productions. That is, plural were productions were an average of 148.2 msec longer in duration, 29.9 Hz higher in peak F_0, 10 Hz higher in mean F_0, 35.9 Hz greater in terms of delta F_0, 0.9 dB higher in peak intensity, 0.2 higher in mean intensity, and 1.7 greater in terms of delta intensity.

A series of one way analyses of variance (ANOVA) for repeated measures (Winer, 1971) were applied to the data to test the observed differences. These statistical analyses indicated that many of the aforementioned differences were indeed significant. Duration ($F = 30.47$; $df = 1, 12$; $p < 0.05$), peak F_0 ($F = 18.58$, $df = 1, 12$; $p < 0.05$), mean F_0 ($F = 11.29$; $df = 1, 12$; $p < 0.05$), delta F_0 ($F = 16.16$; $df = 1, 12$; $p < 0.05$), and delta intensity ($F = 5.54$; $df = 1, 12$; $p < 0.05$) were all greater in the plural form of KM's productions. Neither peak intensity ($F = 1.25$; $df = 1, 12$; $p > 0.05$) nor mean intensity ($F = 0.59$; $df = 1, 12$; $p > 0.05$) were significantly different.

TABLE 1. Means and standard deviations of selected acoustic parameters for singular and plural productions.

	Singular		Plural	
	Mean	SD	Mean	SD
Duration*	257.2	84.3	407.0	113.8
Peak F_0*	264.5	22.4	294.4	25.8
Mean F_0*	233.9	13.2	243.9	11.8
Δ F_0*	56.9	26.5	92.8	37.7
Peak Intensity	48.5	3.3	49.4	3.6
Mean Intensity	46.0	2.2	46.2	2.7
Δ Intensity*	6.2	3.2	7.9	3.0

* denotes significant difference ($p < 0.05$) between singular and plural productions on these parameters.

Discussion

The above analyses indicate clearly that KM had adopted a suprasegmental strategy for marking the singular/plural distinction. This strategy bears a remarkable similarity to those adopted by the language impaired children described within Camarata and Gandour (1985) and within Camarata and Erwin (in press). Because this production now appears to be more widespread than it was originally thought to be (cf. Camarata and Gandour, 1985), the fundamental linguistic nature and phonetic origins of this marking system bear closer analysis. The following morphologic, phonologic, and psycholinguistic factors appear to all contribute to this marking system.

Phonetic Constraints

The shift to suprasegmental marking is phonetically and phonologically reasonable. As noted earlier, segmental control of the phones needed to mark plural emerges relatively late (Ingram, 1976). However, suprasegmental control emerges relatively early in phonological development (Crystal, 1973, 1979). From a phonological perspective, suprasegmental phonemic contrasts are not uncommon in languages of the world. For example, rising versus falling intonation is contrastive in Thai. Similarly, rising (as opposed to falling) intonation is used in English to mark questions (Crystal, 1979). Indeed, young children acquire this intonational form of question marking long before they acquire the syntactic means of signaling questions (Crystal, 1979). Interestingly, a similar pattern of marking was noted by Lord (1974), who indicated that rising versus falling intonation was contrastive for negative marking in the young child she examined. It is clear that a suprasegmental pattern of marking plural is very much in keeping with children's phonetic and phonological capabilities and may indeed a likely form in the early development of this morpheme.

Morphological Constraints

Hockett (1958) reports that number marking is a linguistic universal, indicating the importance of producing the singular-plural distinction within a linguistic system. Evidence of the importance of marking the plural in language acquisition can be seen in the cross-linguistic acquisition data: the plural morpheme is acquired relatively early in most languages (Slobin, 1970). Additionally, the sequence of morphological acquisition in English demonstrates further the relative importance of producing the plural morpheme: The plural is produced earlier than both the possessive and third person singular morphemes (Brown, 1973; James & Kahn, 1982) despite identical phonological structure for all three morphemes.

Beyond this, the plural acquisition also reflects young children's preference for object marking (Slobin, 1979). The plural form is earliest acquired morphological marker involving objects. The importance of this marker can be seen in the fact that possessive, which also involves objects and an identical morphological structure, is acquired much later than plural (Miller, 1981). Clearly, the plural is an important morphological marker in children's linguistic systems and therefore likely to undergo alternative phonological marking.

Psycholinguistic Motivation

Slobin (1979) presents data which indicate an increase in phonetic output is used to signal the plural in many languages. These data, in part serve as the basis for Slobin's language acquisition universal 3: "The closer a grammatical system adheres to one-to-one mapping between semantic elements and surface elements, the earlier it will be acquired (p. 109)." The direction of change, an <u>increase</u> in these suprasegmental parameters, may reflect an increase in the number of objects. The results of this investigation and that of Camarata and Gandour (1985) and Camarata and Erwin (in press) are consistent with this hypothesis: All children within these studies used an increase in the phonetic output to signal plural.

Iconic Aspects of the Marker

In a more recent paper, Slobin (1985) proposes that children are linguistic icon makers: signally perceptual aspects of the language within the actual linguistic structure. For example he notes that negatives are rarely bound morphemes because such elements are not inherent characteristics of the objects and actions whereas possessive are almost exclusively bound morphemes as this marker is included in the noun concept. The results of this investigation and those of Camarata and Gandour (1985) and Camarata and Erwin (in press) are consistent with this hypothesis. Indeed, the plural marker observed here is iconic in the extreme: more objects was associated with an <u>increased</u> magnitude of the suprasegmental parameters. Additionally, the marker appeared as a bound morpheme, again consistent with Slobin's (1985) analysis. Thus, the results of the current investigation can be viewed as strong evidence in favor of the notion that children act as linguistic icon makers.

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Panel

Confessions of a Wayward Chomskyan

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I've been working in this field for almost 30 years, and have seen theories, approaches, and controversies come and go. But the challenges remain those set forth by Chomsky when I was a graduate student in the early 60s. In 1962, when the Ninth International Congress of Linguists met at Harvard and MIT, I heard him pose the challenge in the way that has since become as familiar as passages from the Bible or the Declaration of Independence (Chomsky, 1954, p. 50):

"The central fact to which any significant linguistic theory must address itself is this: a mature speaker can produce a new sentence of his language on the appropriate occasion, and other speakers can understand it immediately, though it is equally new to them. ... On the basis of a limited experience with the data of speech, each normal human being has developed for himself a thorough competence in his native language. This competence can be represented, to an as yet undetermined extent, as a system of rules that we can call the *grammar* of his language."

The extent is still undetermined, and so is the acquisition device. By 1965 the challenge was posed directly to our field, in a famous remark in *Aspects*: "knowledge of grammatical structure cannot arise by application of step-by-step inductive operations...of any sort that have yet been developed within linguistics, psychology, or philosophy" (Chomsky, 1965, p. 58). This, of course, is the argument from "the poverty of the stimulus"--the gap between knowledge and experience. And by 1986, in *Knowledge of Language*, Chomsky's despair with the possibilities of developmental psycholinguistics was fully elaborated (p. 12):

"It seems that there is little hope in accounting for our knowledge in terms of such ideas as analogy, induction, association, reliable procedures, good reasons, and justification in any generally useful sense, or in terms of "generalized learning mechanisms" (if such exist)."

If I may be allowed an analogy, I would characterize this position as "the argument from the poverty of the imagination": Since I can't imagine a reasonable account of language acquisition, no one can.

One must not minimize "Plato's problem" ("the problem of explaining how we can know so much given that we have such limited evidence" [Chomsky, 1986, p. xxv]), but neither should one shy away from the task of studying the unfolding relationships between knowledge and evidence over time. It has been our attempt, at Berkeley, to press our imaginations--as developmental psychologists, rather than as linguists--in pursuit of childlike means of the growth of knowledge. Here I speak for Susan Ervin-Tripp and myself and 20 years of students. Our attempt has been to start from individual children and individual languages, rather than a preconceived view of universal grammar, trying to characterize the strategies or "operating principles" that children use in building up grammars within the contexts of developing cognition and communication. In the course of designing and testing Operating Principles against acquisition data in different languages, I believe we can arrive at an empirically grounded definition of the innate competence that guides the child in the construction of grammar.

But that is not all--because our interests are broader than syntax, and because we are not in a hurry to have the child arrive at the end-state without an interesting journey along the way. This is because we take a *psychologist's* concern with *development* as an important and interesting process in its own right. At Berkeley, we do not see the study of child language as derivative from syntactic theory. We are convinced that if we attend only to the adult model, we will fail to discover psychologically valid *interim solutions* attempted by the child in the construction of grammar. Interim solutions, we believe, can reveal basic aspects of the human language-making capacity. In fact, as I tried to show in my Stanford keynote address some years ago (Slobin, 1977), one can learn things about studying language in processes of change that may not be as clearly revealed in synchronic studies.

I must admit, though, that we have other grounds for a less-than-romantic relationship with formal syntax. We have been on the scene long enough to watch popular syntactic theories come and go at an alarming rate, leaving little of cumulative significance in the field of child language. I am reminded, most poignantly, of the years of careful work of my teacher, Roger Brown, in preparing the second volume of *A First Language*--which never appeared, because, by the time he was ready, the *Aspects*-type grammars he had prepared for Adam, Eve, and Sarah were no longer of current interest or repute. In his "Autobiography in the Third Person," published this year, Brown reports the difficulties of a psycholinguist who had wedded himself to a particular phase of syntax (Brown, 1988, pp. 398-399):

"The planned second volume of *A First Language* that was to cover *The Later Stages* was never written. People used to ask about it but after several years that became embarrassing and developmental psycholinguists came to assume that it never would appear." Why has it not? Data collection had been complete in 1973 and so had data description in the form of unpublished grammars. Brown had an unhappy sabbatical year in which he worked hard on *The Later Stages* but finally had to admit defeat. The detailed analyses of presumptive Stages III, IV, and V did not yield up to Brown, then, any strong generalizations comparable to those of the early stages, and he could see no value in publishing the possibly quite idiosyncratic details available in the unpublished grammars. In addition, linguistic theory was evolving rapidly and Brown, never quick at learning new formalisms, could hardly keep up.

I also learned something from being raised, in the 50s, on the learning theories of Hull, Tolman, Guthrie, and Skinner; the following cognitive revolution; and the cyclic domination of nativism and empiricism in psychology. Again, Brown's aesthetic sensitivity has been a guide. In 1977 he commented (quoted by Pinker 1988, p. 117):

"Developmental psycholinguistics has enjoyed an enormous growth in research popularity. ... All of which, strange to say, may come to nothing. There have been greater research enthusiasms than this in psychology: Clark Hull's principles of behavior, the study of the authoritarian personality, and...dissonance theory. And in all these cases, very little advance in knowledge took place. ... A danger in great research activity which we have not yet surmounted, but which we may surmount, is that a large quantity of frequently conflicting theory and data can become cognitively ugly and so repellent as to be swiftly deserted, its issues unresolved."

To be quite frank, much of current linguistics and psycholinguistics strikes me as due for this fate. But Brown went on to say:

"But in the end, the thing I credit most for what successes the field has had and the thing I most count on is that we are somehow lucky in our subject matter: there are astonishing regularities in child speech and some are very near the surface; a little deeper, I feel sure, are real laws."

I share this hope. We *have* discovered many "astonishing regularities," crosslinguistically, in the past 20 years. Many of them appear to be "very near the surface," yet promise to yield up deeper insights.

And I think that most of these regularities have been discovered from detailed observations of individual children acquiring individual languages. The most essential research tool remains exhaustive, longitudinal case studies of strategically selected languages, supplemented by artificial probes (what we self-consciously call "experiments") of dimensions of children's knowledge and use of language. We will not suffer the fate of the great programs in psychology that Brown recalled, because our data are taken from natural phenomena, rather than artificial laboratory or pencil-and-paper tasks. We do have a growing, cumulative, crosslinguistic database that will outlive our current poor attempts at theorizing, and which should serve future theorists.

In my own work, I believe it is possible to abstract from this database to arrive at general *Operating Principles* for language acquisition--strategies for language construction--along with a characterization of the child's natural and growing organization of semantic and phonological space, and basic syntactic principles. This work is closely related to Operating Principles elaborated by Brian MacWhinney (1985) and by Ann Peters (1985), and has much in common with what Steve Pinker (1984) calls "procedures" in his book on learnability. In my 1985 book, *The crosslinguistic study of language acquisition*, I began my chapter on Operating Principles with a sort of "credo" of my position, which I will take the liberty of quoting in part (pp. 1158-1159):

"In one way or another, every modern approach to language acquisition deals with the fact that language is constructed anew by each child, making use of innate capacities of some sort, in interaction with experiences of the physical and social worlds. ... It is only by detailed examination of patterns of children's verbal interaction with others that we can form a picture of the child's activity in constructing language. By observing repetitions of such patterns across individual children and languages we can begin to form hypotheses about the underlying capacities that may be responsible for language acquisition in general. ..."

"... Rather than "pre-tune" LMC [the Language-Making Capacity] to a particular current theory of abstract syntax, I prefer to work backward from acquisition data to propose systems of knowledge and information processing that seem to be prerequisite for the sorts of data that we encounter crosslinguistically. Clearly, LMC must begin life with some initial procedures for perceiving, storing, and analyzing linguistic experience, and for making use of capacities and accumulated knowledge for producing and interpreting utterances. I believe that we do not know enough yet about LMC to be very clear about the extent to which it is specifically tuned to the acquisition of language as opposed to other cognitive systems, or the degree to which LMC is specified at birth--prior to experience with the world of people and things, and prior to interaction with other developing cognitive systems. These issues are full of controversy precisely because we know so little about LMC and comparable capacities for the acquisition of other forms of structured knowledge and behavior. The only way that we can ever gain more clarity about issues of innateness and task-specificity is to obtain considerably more detailed descriptions and theoretical accounts of the course of development of language and of other systems. In spite of the many pages of this book, and many other publications, it is evident that we have only the most preliminary understanding of LMC, and it is difficult to find comparable treatments of other aspects of development. In this chapter, therefore, I try to pull together what is suggested by current crosslinguistic comparison in regard to the nature of LMC, leaving it to future scholars to find a place for this capacity in a broader theory of the mind and its development. ..."

"The task, then, is to propose a set of procedures for the construction of language. I have used the term "Operating Principle" (OP) to denote the "procedures" or "strategies" employed by LMC (Slobin, 1971, 1973, 1982). OPs, whatever their ultimate origin, are necessary prerequisites for the perception, analysis, and use of language in ways that will lead to the mastery of any particular input language. The postulation of OPs constitutes a psycholinguistic attempt to respond to the challenge of Chomsky's claim that "knowledge of grammatical structure cannot arise by application of step-by-step inductive operations ...

of any sort that have yet been developed within linguistics, psychology, or philosophy" (1965, p. 58). The goal of this chapter is to propose a set of OPs based on the crosslinguistic evidence currently available, trying to sort out the ways in which knowledge of grammatical structure is given in advance and the ways in which it is constructed in the course of linguistic, cognitive, and social experience"

Working within such an approach, it has been possible to account for the development of morphological paradigms, canonical sentence forms, placement of operators, and various patterns of over- and underextension of meaning. In an exploratory study with Katherine Demuth and Ruth Miller it has even been possible to *predict* some acquisition patterns in a previously uninvestigated Bantu language, Sesotho. I think we've been especially successful in isolating some of the perceptual factors that make grammatical morphemes salient to children, along with the basic semantic notions expressed by such morphemes. The Operating Principles require a primordial specification of linguistic units and categories, along with a richly structured semantic space, in which concepts are arrayed in terms of proximity to one another and accessibility to the learner.

Beyond the Operating Principles, a new approach has been developing at Berkeley--one that I consider especially promising. We have always been interested in the semantic bases of the use of grammatical morphemes. However, recent dissertations by Julie Gerhardt (1983), Iskender Savasir (1984), and Nancy Budwig (1986) have shown that grammatical morphemes emerge in specific interactional contexts that must be characterized in both semantic and pragmatic terms. To give one brief example, from Nancy Budwig's 1986 dissertation: some 2-year-olds make a systematic distinction between *I* and *my* as subject pronouns. These children use *my* in utterances in which the subject is a prototypical agent, with a highly kinetic verb and a direct effect--either to report a completed volitional act, such as "My blew the candles out," or to announce such an act, as in "My take it home." Thus *my* tends to co-occur with verbs that are either past-tense and perfective, or future-intentional. When *I* is used as subject pronoun, the utterances are low in agentivity, expressing experiential states, such as "I like peas," in response to an adult question. Budwig concludes that semantic and pragmatic factors function jointly to determine such idiosyncratic pronominal uses: "The uses of *my*...appear in utterances that function as Control Acts: that is, as directives, requests, challenges, protests and disputes over control of objects and enactment of activities. ... In contrast, utterances ranking low in transitivity involving the use of *I* involve no such attempt to bring about a change." Such "semantic/pragmatic" constellations do not play a role in the end-state grammar, but they are important in understanding development, and, I believe, they play a role diachronically in accounting for patterns of grammaticization in languages.

The Berkeley studies of the 80s are informed by Susan Ervin-Tripp's wise insistence that grammatical forms be considered in their contexts of use. Currently we are examining the emergence and refinement of tense/aspect markers, relative clauses, and the syntax of temporal and causal subordination in the contexts of narrative, studied crosslinguistically and across a large age span, from 3 to 11 (Slobin, 1987). It is becoming evident that linguistic forms and constructions have a long developmental history that cannot be understood without attention to their discourse functions.

Finally, we are beginning to see how the grammatical forms of particular native languages predispose children to take one or another perspective on events in narrative, raising old questions of the influence of language on thought. I could say more--especially about the links we are discovering between child language and historical language change, and hints of universal cognitive bases of grammaticization that can be studied in ontogeny and linguistic diachrony. But this would lead me far afield in these brief remarks. In conclusion, I think that there is a healthy and growing attention to *mutual* relations between form and function in language. For my part, I will continue to train my students in the

study of many languages, in their discourse contexts--while keeping an ever-hopeful eye open to promising developments in linguistics.

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Panel

WHAT IS FUNCTIONALISM?

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For the last fifteen years, we have been involved in collaborative research on language acquisition in children and language processing in adults, across a range of structurally and functionally distinct language types (Bates and MacWhinney, 1979, 1982, 1987, in press; MacWhinney, 1987; MacWhinney and Bates, in press). We have brought those findings together within a framework for the study of linguistic performance called the *Competition Model*, a model that is in turn inspired by a broader approach to the study of language called *functionalism*, defined as the belief that *"the forms of natural languages are created, governed, constrained, acquired and used in the service of communicative functions"* (Bates and MacWhinney, 1982). So defined, functionalism is the natural alternative to theories of language that postulate a severe separate between structure and function, and/or theories that attempt to describe and explain structural facts *sui generis*, without reference to the constraints on form that are imposed by the goals of communication and the capabilities and limitations of human information processing.

Although this definition seems sensible enough as stated, it has become sadly clear to us over the years that the term "functionalism" does not communicate very well on its own. It means different things to different people, and worst of all, there seems to be a Straw Man Functionalism out there in the hustings that causes trouble wherever we go. In this short paper, we would like to compare and contrast the principles of Straw Man Functionalism with an approach that is (we believe) much more reasonable and much more likely to succeed. The Straw Man theory can be summarized with the following six beliefs:

(1) **Grammar is a direct reflection of meaning.** That is, we can explain all universal and particular aspects of grammar by uncovering the meanings they convey.

*Portions of this paper are taken from E. Bates & B. MacWhinney (in press), "Functionalism and the Competition Model". In B. MacWhinney & E. Bates (Eds.), *The cross-linguistic study of sentence processing*. New York: Cambridge University Press.

(2) **Grammar is iconic.** That is, grammatical devices "look like" their meanings.

(3) **Mappings from meaning to grammar are one to one.** For every meaning there is one and only one expressive device, and for every device there is one and only one associated interpretation.

(4) **Mappings from meaning to grammar are deterministic.** If the meaning conditions associated with a given grammatical form are met, the form will always be used.

(5) **Functionalism is anti-nativist**. Grammars are a cultural invention, and biological principles are irrelevant to their description and explanation.

(6) **Functionalism is anti-linguistic.** Functionalist theories of human performance will ultimately replace linguistic theory altogether. We will bury Chomsky and all the other generative grammarians with him!

In fact, we do not believe that any of the above six statements are true, and we have never espoused them ourselves. So let us go through these six Straw Beliefs one at a time, and replace each one with a more viable functionalist account.

(1) Grammars reflect the interaction between cognitive content and cognitive processes. We believe that grammars carry out important communicative work. Like individual lexical items, specific grammatical devices (ordering constraints, bound and free morphemes, suprasegmental cues) are associated with meanings and/or communicative goals. But the association is rarely direct. We think it more useful to think of language as a complex, multivectorial problem space. Many different meanings are competing for expression in a linear (i.e. time-delimited) channel. The limits imposed by human information processing (limits of perception, articulation, learning and memory) may

ultimately prove more important than meaning itself in elucidating why grammars come to look the way they do.

In the Competition Model, we have borrowed the term *cue validity* to refer to the information value of a given lexical or grammatical device for any particular meaning or function. The term comes from Gestalt psychology, where it was broadly used to refer to the information structure of some aspect of the environment for any goal or condition that is of interest to the organism. In an ideal world, an ideal animal would behave in perfect accordance with cue validity. But we do not live in an ideal world, and we are not ideal animals. The relationship between meaning and form in language cannot be perfect, because of all the constraints imposed by our information processing system. Our experiments to date have shown that cue validity strongly determines the order of acquisition of cues by children, and the weights that adult speakers attach to the same cues during sentence interpretation. Cue validity also plays a major role in the sentence comprehension and production profiles displayed by brain-damaged adults suffering from severe forms of aphasia. However, there are still many systematic exceptions to this principle. We have been able to account for most of these exceptions by invoking principles of *cue cost,* i.e. the information processing costs associated with the real-time use any given lexical or grammatical cue. For example, cues that are equally informative can vary in their perceivability (e.g. Hungarian accusative case suffixes that follow a strong vowel, compared with the same suffix following a final consonant). This factor will influence that degree to which adults "trust" this particular cue to meaning, the age at which children come to rely on the cue, and the degree of resistance to impairment associated with this particular cue in sentence processing by brain damaged adults. Similarly, cues can vary in the demands they place on memory: "local" cues that can be used as soon as they are encountered (e.g. a nominative case suffix) seem to have an advantage over "long distance" cues that require storage and comparison across a set of discontinuous elements (e.g. subject-verb agreement), even though the two sets of grammatical devices may both point strongly toward the same meaning (e.g. the actor role in a transitive action). A full account of how grammars come to look the way they do, how and when they are acquired by children, will require an analysis of the complex interplay between meaning (cue validity) and information

processing (cue cost). Grammars represent a compromise among these forces, and for this reason, the communicative function of a given grammatical form may be quite opaque.

(2) Symbolic and indexical relations between form and function. Linguistic forms rarely, if ever, resemble their meanings. There are of course a few examples of words that "sound like" the things they stand for (e.g. Bang!), but these are few and far between. It is even more difficult to think of grammatical devices that bear a literal physical resemblance to their meanings. There is of course the apocryphal claim that natural languages prefer basic words orders in which the subject precedes the verb because human beings "naturally" tend to perceive actors before they perceive their actions. This claim is silly enough that it is not worth pursuing. But if grammars do not "look like" their meanings, then what kind of natural cause-and-effect relationship could be said to hold between form and function?

C. S. Peirce (1932) has provided an analysis of sign-referent relations that may be as useful in the study of grammar as it is in the study of single signs. *Icons* are signs that come to stand for their referents because of a literal physical resemblance (e.g. a stylized picture of a cigarette to indicate a smoking zone). *Indices* are another class of "natural" signs that come to stand for their referents not because of a physical resemblance, but because their participation in the same event (e.g. contiguity rather than similarity). For example, smoke can serve as an index to fire because the two are commonly associated in real life. *Symbols* are signs that bear no natural relation to their referents (neither iconic nor indexical); instead, they carry meaning only because of an arbitrary convention, an agreement that was reached by a particularly community of users. As Langacker (1987) has pointed out, most lexical and grammatical signs bear a symbolic relationship to their meanings. Grammatical devices exist in order to carry out communicative work, but the work they do does not determine their form. However, in the domain of grammar there may well be many cases of indexical causality if we keep in mind that grammars are jointly caused by cognitive content *and* cognitive processing.

To offer just one example, consider the relative clause. This device is typically used to identify referents in discourse (e.g. "The man that sold me the car", as opposed to some other man), a functional motive constitutes in itself only a form of symbolic determinism. However, the functions served by a relative clause can also help to determine its shape. Bindings between a referent and its modifier are easier to make if the two are in close proximity. Hence the function of referent-identification is best served if the relative clause is placed near its governing noun phrase, where other modifiers are located. However, this solution usually poses another problem: the relative clause must interrupt a main clause. Such interruption is costly for two reasons. First, because relative clauses are longer than most modifiers, the main clause has to be held open for a rather long time. Second, because relative clauses resemble main clauses in many respects, there is a potential for confusion (e.g. which verb goes with which noun). In principle, this problem could be solved by placing a warning signal at the beginning of a sentence to indicate that "a relative clause will be placed within the following sentence at some point; you guess which point". Although this is a logical possibility, it should be obvious why it would not work very well. It makes much more sense to place the marker *at the point of interruption*, to keep the listener from chasing down some garden path and to help him/her construct and attach the clause right where it belongs (i.e. near the element that it modifies). Finally, insofar as an interruption is already placing quite a burden on the processor, the interruption-marking device had best be kept short and sweet. Hence the functions of the relative clause have an effect not only on the existence of certain devices (symbolic determinism), but also on their position and overall shape (indexical determinism). In neither case is it reasonable to say that the resulting grammatical device "looks like" its meaning!

(2) **Mappings between form and function are many-to-many.** Grammars can be viewed as a class of solutions to the problem of mapping non-linear meanings onto a highly-constrained linear medium. The universal and culture-specific contents of cognition interact with universal constraints on information processing, creating a complex multivectorial problem space with a finite number of solutions. Natural languages exhaust the set of possible solutions to this mapping problem, and because these solutions represent many competing forces, they

invariably involve many-to-many mappings between form and function (c.f. Karmiloff-Smith, 1979), with correlated meanings riding piggy-back on correlated bits of grammar. No single meaning (however abstract) can be allowed a grammatical monopoly.

The many-to-many nature of grammatical mapping is both a cause and a result of the instability inherent in linguistic systems. In fact, there may be no stable, perfect pathway through the linguistic problem space. As Slobin (1982) has pointed out, many processing constraints stand in direct competition; hence stability in one area may create instability in another. From the listener's point of view, a given linguistic marker will signal its meaning most efficiently if it is consistent, salient and unique. But from the speaker's point of view, the same linguistic device has to be easy to retrieve and produce. Hence the clear and perceivable markers that evolve for comprehension are often subject to erosion in the service of rapid and efficient speech output. Faced with these competing demands, languages have been known to cycle back and forth across the course of history, from one set of solutions to another. Hence we must view grammars as a set of *partial solutions* to the mapping problem, each representing one pathway through the constraints imposed by cognitive content and cognitive processing. No solution is perfect, and each one is constantly subject to change; but every grammar used by a community of human adults and acquired by their children has to meet certain some implicit but implacable limits of tolerance.

(4) Grammatical mappings are inherently probabilistic. Languages differ qualitatively, in the presence or absence of certain linguistic devices (e.g. word order constraints, case-marking), but they also differ quantitatively, in the extent to which the "same" linguistic device is used at all and in the range of functional roles that the "same" linguistic device has come to serve.

We have given a number of examples of quantitative differences between languages throughout our work (see especially papers in MacWhinney and Bates, in press). One particularly important example has to do with the relative strength of word order versus subject-verb agreement as cues to sentence meaning. In English, word order is rigidly preserved in almost all structures (we will consider a few exceptions

latter), the order that is preserved is Subject-Verb-Object or SVO. In Italian, word order can be varied extensively for pragmatic purposes -- a fact that comes as something of a surprise to those who believe that such pragmatic word order variation occurs only in case-inflected languages (i.e. languages with markers on the noun to indicate "who did what to whom"). The following list (from Bates and MacWhinney, in press) illustrates some possible variations in the order of major constituents in Italian, in a hypothetical restaurant conversation. This short conversation (a fake, but quite plausible according to our Italian informants) contains all possible orders of Subject, Verb and Object.

1. **SVO:** **Io mangerei un prima.** (I would eat a first course).

2. **OSV:** **La pastasciutta Franco la prende sempre qui.** (Pasta Franco it orders always here).

3. **VSO:** **Allora, mangio anche io la pastasciutta.** (Well then, am eating also I pasta).

4. **VOS:** **Ha consigliato la lasagna qui Franco, no?** (Has recommended the lasagna here Franco, no?).

5. **OVS:** **No, la lasagna l'ha consigliata Elizabeth.** (No, the lasagna it has recommended Elizabeth).

6. **SOV:** **Allora, io gli spaghetti prendo.** (In that case, I the spaghetti am having).

Some of these require particular intonation patterns to sound exactly right, and some are definitely better with particular grammatical markers like the object clitic. But all these orders can be found in a large enough sample of free speech, and all of them occur at some point in the input received by Italian children (Bates, 1976).

At one level, this discourse serves merely to illustrate a well-known qualitative difference between languages. Italian has word order options that do not exist in English at all. However, this qualitative variation also has quantitative implications. We have now demonstrated in

several different experiments that Italian listeners "trust" word order -- even good old-fashioned Subject-Verb-Object order -- less than their English counterparts. Given a sentence like "The pencil hits the cow", English listeners from ages 2 to 80 have a strong tendency to pick the pencil as the agent/subject. Given the Italian equivalent ("La matita colpisce la vacca"), Italians are much more likely to choose the cow as the agent/subject. Hence a qualitative difference in the availability of word order types has a quantitative effect even on that subset of grammatical structures that both languages share (e.g. SVO order).

Most of our joint research to date has concentrated on sentence comprehension. But we have also uncovered some interesting quantitative differences in the domain of sentence production. For example, Bates and Devescovi (in press) have described some robust differences between Italian and English in the use of relative clauses. The structural options available in the two languages are the same, at least for the set of structures studied by these investigators. In both languages, it is perfectly grammatical to describe a picture of a monkey eating a banana by saying either "A monkey is eating a banana" or "There is a monkey that is eating a banana". However, English speakers typically use the first option; by contrast, Italian speakers describing exactly the same pictures, under the same conditions, are three to five times more likely to produce a relative clause. This cross-linguistic difference in relative clause use is already well-established in children by the age of three, and it tends to persist even in elderly patients who have suffered left-hemisphere damage. How can we capture a quantitative difference between two structures that are equally grammatical from a traditional grammatical perspective? To be sure, there are some differences between the two languages in the range of functions that control these particular forms. In particular, Italians appear to use the relative clause as a kind of topic marker. But in addition to (and perhaps because of) these differences in function, there are also clear processing differences between English and Italian in the "accessibility" of the relative clause. We have uncovered similar statistical differences between Italian and English children in rates of article omission (greater in English children well before the age of 2), and in rates of subject omission (with much higher rates of subject omission in Italian children even in the stage of first word combinations -- Bates, 1976). Some of these differences (e.g. subject omission) are

treated in current linguistic theory in terms of a discrete set of rules or parameters; others (e.g. article omission) receive no treatment in current linguistic theory at all. We think that these early differences in performance can only be captured by assuming that very small children are sensitive to statistical as well as structural facts about the language they are trying to acquire. Function and frequency co-determine the selection of grammatical forms in sentence production, in language use by adults and in language acquisition by children.

Physicists have made their peace with the counter-intuitive predictions of quantum mechanics, and they now accept the premise that the position of a subatomic particle may be unknowable *in the absolute*. Uncertainty lies at the core of the universe; it is not just a byproduct of our imperfect measures. We argue that the human language processor is also probabilistic at its core. In the Competition Model, the adult speaker's knowledge of his native language is represented in a probabilistic form, and probabilities play a fundamental role in the process of language acquisition. The difference between obligatory rules and statistical tendencies is simply a matter of degree. This does not mean that we ignore the powerful laws that separate one language from another. After all, the values "0" and "1" do exist even in a probabilistic system, and an adult native speaker may thus come to know with some certainty that a particular structure is impossible in his or her language. The difference between our characterization of adult knowledge (i.e. "competence to perform") and the characterizations offered in most competence models lies in our ability to capture the many values that fall between 0 and 1. We describe linguistic representations in terms of a complex set of *weighted form-function mappings*, a dynamic knowledge base that is constantly subject to change.

In a sense, language acquisition can thus be viewed as a process of *meaning driven distributional analysis*, similar in spirit to the approach outlined some time ago by Maratsos (1982). However, the Competition Model also furnishes some non-linear principles that permit us to capture sudden phase transitions, U-shaped functions, and the effect of rare events -- all the phenomena that forced psychologists to abandon the simple linear associative models of American Behaviorism. Many of these discoveries within our model have fallen out of two approaches to the

quantification and formalization of language learning: (a) mathematical modelling of the effects of cues on choice behavior in sentence comprehension (McDonald, 1986; McDonald and MacWhinney, in press), and (b) computer simulations of the learning process (Taraban, McDonald and MacWhinney, in press). For example, we have discovered that cue validity can be operationalized in two ways: *overall cue validity* (the proportion of all the cases in which an interpretation must be made in which a given cue is available and leads to a correct interpretation), and *conflict validity* (the proportion of cases in which one cue competes with another in which the cue in question "wins"). Both these metrics can be calculated objectively from texts of real speech, and used to predict the choice behavior of children and adults in sentence comprehension experiments. Interestingly, we have discovered that overall cue validity drives the early stages of language acquisition; conflict validity (affected primarily by rare cases, particularly those that are encountered in complex discourse) drives the late stages of learning in older children and adults. With these two statistical principles, we can capture abrupt changes in sentence processing strategies that occur as late as 7 - 10 years of age.

Although the Competition Model has been developed on independent grounds (to deal with facts of acquisition and processing across different natural languages), the model in its current form has a great deal in common with a recent movement that is alternatively referred to as *connectionism*, *neural modelling* and/or *parallel distributed processing* (e.g. Rumelhart, McClelland and the PDP Research Group, 1986; Elman, 1988). It remains to be seen how strong that relationship will be, but we are at least convinced that the tools we share will prove to be exceptionally important in the next era of language acquisition research. Cognitive psychology has proceeded for more than thirty years without an adequate model of learning. Unfortunately, research in language acquisition has done the same. The new focus on learning in "brain-like systems" is a healthy one, whatever its limits may prove to be. And the new tools (i.e. mathematical modelling, multivariate statistics, computer simulation) are bound to lead to progress. Natural languages are so complex that "eyeball analysis" alone can only take us so far -- probably no farther than we have come to date.

(5) Functionalism is biologically plausible. The innateness issue is one of the major sources of anger and misunderstanding in the field of psycholinguistics. We think that much of this misunderstanding comes from a failure to distinguish between *innateness* and *domain-specificity*. The innateness issue has to do with the extent to which human language is determined by the unique biological heritage of our species. But this biological heritage may include many capacities that are not unique to language itself: our large and facile brain, our particular social organization, our protracted infancy, and a variety of unknown factors that may contribute in indirect but very important ways to the problem of mapping universal meanings onto a limited channel, and to the particular solutions that we have found to that problem. Hence the human capacity for language could be both innate and species-specific, and yet involve no mechanisms that evolved specifically and uniquely for language itself. Language could be a new machine constructed entirely out of old parts (Bates, 1979). The universal properties of grammar may be *indirectly innate*, based on interactions among innate categories and processes that are not specific to language. In other words, we believe in the innateness of language, but we are skeptical about the degree of domain-specificity that is required to account for the structure and acquisition of natural languages.

(6) Functionalist claims are made at different levels. Functionalist theories of performance are not in direct competition with any linguistic theory. Different kinds of functionalist claims require different kinds of evidence. This is a point that we have tried to make in several places (notably Bates and MacWhinney, 1982; Bates and MacWhinney, 1987 and in press), but it is sufficiently important that we think it deserves reiterating here. We distinguish four different levels of functionalist claims, ordered from weakest to strongest (in the sense that claims at the higher levels presuppose that claims at the lower levels are true).

Level 1 focusses on the role of cognitive and communicative functions in the evolution of language proper, and the history of individual languages. Claims at Level 1 constitute a kind of linguistic Darwinism, i.e. arguments that functional constraints have played a role in determining the forms that grammars take today. Where did the tiger get his stripes? Why do

grammars have relative clause markers? A great deal of work in functionalist linguistics is of this historical sort, in particular studies of "grammaticization" (e.g. Givon, 1979; Bybee, 1985). Although this work is extremely interesting in its own right, claims at the historical level have no necessary implications for current language use by adults, language acquisition by children, or the proper characterization of grammatical knowledge. Like the large-scale forces that operate to create mountains and rivers across geological time, the forces that operate across many individuals to bring about historical language change may not be detectable (or even operative) in every individual case.

Level 2 is a synchronic variant of Level 1, focussing on the causal relationship between form and function in real-time language use by adult speakers of the language. Much of our own work with adults is of this sort: we manipulate competing and converging sets of grammatical forms as "causes" to see what interpretations our subjects derive; conversely, we manipulate competing and converging meanings in picture and film description, to see what expressive devices our subjects produce to meet these demands. However, even if we could show a perfect cause-and-effect relation in adults, we could not immediately conclude that children are able to perceive or exploit these relations.

Level 3 presupposes but goes beyond Level 2, focussing on the causal role of cognitive and communicative functions in language acquisition by children. The cause-and-effect work of Level 2 must be repeated at every stage of language acquisition, to determine empirically if and when children are sensitive to the form-function correlations available in the adult model. Furthermore (as we noted earlier), we need a well-articulated theory of the learning process, one that can adequately describe, predict and explain the stages that children go through on their way to adult performance.

Finally, Level 4 is reserved for the claim that facts from Levels 1 - 3 play a direct role in the characterization of adult linguistic competence. A variety of competence models of this sort have been proposed within the functionalist tradition, ranging from *Eastern European functionalism* (i.e. the so-called Prague School -- Dezso, 1972; Driven and Fried, 1987; Firbas,

1964; Firth, 1951), *British functionalism* (e.g. Halliday, 1966), the American school of *generative semantics* (e.g. Fillmore, 1968; Chafe, 1971), to more recent proposals that include *cognitive grammar* (Langacker, 1987; Lakoff, 1987), *construction grammar* (Fillmore, 1987), *role and reference grammar* (Foley and Van Valin, 1984), and several other approaches that either retain the simple term "functionalism" or elect to avoid labels altogether (e.g. Dik, 1980; Kuno, 1986; Givon, 1979. For the sake of simplicity, we will refer to these otherwise rather disparate linguistic theories with the single term *functional grammar*. Although functional grammars are not designed to account for real time processing, they are most compatible with highly interactive models of performance, i.e. with models like ours. For obvious reasons, "modular" theories of performance are instead more compatible with "modular" theories of competence, that is, with linguistic theories that emphasize the autonomy of various components and subcomponents of the grammar (c.f. Berwick and Weinberg, 1984; Bresnan, 1982; Pinker, 1984). It is quite possible that there will ultimately be a convergence between some Level 4 version of functional grammar, and the performance model that we have developed to account for data at Levels 1 to 3. But it is also possible, at least in principle, that there may be a rapprochement between a functionalist model of performance and the various rules and representations that have been proposed within the many-times-revised-and-extended school of generative grammar.

In short, we are not anti-linguistic, nor is our work *directly* relevant to any particular class of competence models. We are consumers of linguistic theory, and we have our own bets about which linguistic theory or class of theories will ultimately prevail. But we are much too preoccupied with problems of a different sort to enter into the linguistic fray. This is an exciting new era in language acquisition research, and time is too precious to be wasted on battles that are best waged elsewhere.

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Panel

A Principles-and-Parameters Approach to the Study of Child Language

(or 'why isn't language acquisition instantaneous?')

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The Logical Problem

In order to solve the language acquisition puzzle there are two basic problems one needs to address. The first is the so-called "logical problem of language acquisition" and the second is what I will refer to as the "developmental problem of language acquisition." The logical problem of language acquisition is the question, no doubt familiar to you all, of how we account for the richness, complexity and specificity of our shared linguistic knowledge given the limitations of the available data. For example, how do we all know that in the sentence in (1a) Michael and he may refer to the same individual, but in (1b) they may not?

(1) a. Michael said that he was hungry.
b. He said that Michael was hungry.

Obviously, none of us are instructed in such matters and it seems clear that the kind of information which would be necessary for a child to learn such a restriction is not available in any form in the input. Thus, in answer to the logical problem of language acquisition, linguistic theory proposes that the child is endowed with a richly articulated set of innate, specifically linguistic principles. These principles interact with linguistic input to determine a particular adult or target grammar. As you know, this system of innate knowledge is referred to as UG (Universal Grammar) (Chomsky, 1965).

Within current theories of grammar UG is formulated as a parameterized system. That is, there is a set of universal principles, some (or perhaps all) of which have associated with them parameters. Each parameter expresses the limited range of variation that languages exhibit with respect to the principle. Let me give some examples. There is a grammatical principle which specifies that phrases are "endocentric" or headed (Stowell, 1981). Thus, VP contains V, NP contains N and so on. Languages vary, however, with respect to the position of the head within its phrase. Thus, there are left-headed languages such as English in which the head precedes its complements and right-headed languages such as Japanese. By hypothesis the child has prior knowledge of the endocentric requirement. His or her task is to determine the position of the head, first or last, within its phrasal projection. This parameter is set for the child's particular

language based on certain triggering data in the input.

There is also a system of parameters associated with the Binding Theory. Binding Theory consists of a set of principles specifying the domain within which anaphors and pronominals may be referentially dependent on antecedents. The binding principles state roughly that an anaphor must be bound to an antecedent within a specified syntactic domain D, while a pronoun must be free of an antecedent within some specified domain. However, the value of D varies within limits across different languages. Thus, in English D is the minimal S or NP containing the anaphor or pronoun, while in Icelandic D is the minimal indicative clause. In Icelandic then, the reflexive may in certain instances have an antecedent in a higher clause as illustrated in (2).

(2) Eirikur segir ad Maria elski sig
'Erik says that Maria loves (subjunctive) himself'

Again, the child is faced with the task of determining what the binding domains are for anaphors and pronouns in the particular language he or she is born into.

To take one final example, we have the so-called Null Subject Parameter. UG specifies that all sentences must have subjects; however, languages vary according to whether the subject need be phonologically specified or not. In English and French, for example, a lexical subject is obligatory as illustrated in (3a,b), while in Spanish, Italian, and Chinese it need not be expressed, as shown by the examples in (4).

(3) a. *(I) eat rice.
b. *(Je) mange du riz .

(4) a. (Yo) como arroz.
b. (Io) mangio risotto.
c. (Wo) chr fan.

There are a number of different proposals concerning the precise formulation of the Null Subject Parameter (cf. Jaeggli & Safir, forthcoming). For our present purposes, it is sufficient to note the that choice of one or the other option made available by the parameter gives rise to a number of grammatical properties which distinguish null subject from non-null subject languages. As in the previous cases, the child's task is well-defined. He must set the Null Subject Parameter at the value which is appropriate for the language of his speech community.

From the point of view of linguistic theory the language learner comes to the acquisition task with a questionnaire: Does my language have null subjects? Do complements precede or follow the head within XP? Can anaphors be bound outside a non-local domain, and so on. Ideally, each of these questions can be answered on the basis of readily accessible positive evidence in the input. Once all these questions have been answered, that is once the parameters have been set, the child has the adult grammar of the language, or at least what

is known as the 'core' grammar (Chomsky, 1981).

The Developmental Problem

Assuming, as I do, that this picture accurately reflects the basic character of grammatical development, we are presented with the following problem. Linguistic theory treats language development as an 'instantaneous' process, which is to say that it idealizes to a situation in which the child has at his disposal all of the principles and parameters of UG and all linguistic data necessary to fix those parameters. But of course, language acquisition is not an instantaneous event and thus we must explain the developmental sequence which ultimately terminates in an adult grammar. This is the second part of the acquisition puzzle which I referred to earlier as the developmental problem.

What then is the relationship between the logical problem of language acquisition and the developmental problem? As I see it, the logical problem and the developmental problem are really two sides of the same coin. Both are concerned ultimately with explaining how the child arrives at an adult grammar. The theory of grammar attempts to explain the apparent ease, rapidity and uniformity of acquisition in the face of impoverished data, (what is often referred to as 'Plato's Problem' (Chomsky, 1986)), while the developmental theory must explain the apparent 'difficulties' which the child encounters and the various 'delays' which characterize the developmental process. In other words, it is the task of the developmental theory to explain those factors which make acquisition 'non-instantaneous' and much of the current research within the principles-and-parameters framework is concerned with precisely this question.

What then accounts for the lack of instantaneity in grammatical development? As one might expect, there are a number of contributing factors. First, as noted earlier, the idealized language learner has access to all of the principles and parameters of UG as well as all of the triggering data. In actual acquisition, however, all of this information may not be immediately available to the child. We know that children are selective in the data they attend to at any point of development. This being the case the child may not in fact have all of the relevant data at his disposal at any one time resulting in real-time delays. As White (1981) has suggested, we may need to distinguish between 'input' data, which is always available in the environment, and 'intake' data, the data which the child is able to perceive at a particular point in development.

There is also some evidence that not all of the principles of UG are available at the initial state. There appears to be a maturational schedule according to which certain principles of lesser energy at later points in development. For example, Borer and Wexler (1987) have argued that there is a specific linguistic principle which must mature before the child's grammar licenses NPs which

have undergone movement." Prior to this maturational point children will not produce or interpret verbal passives or raising constructions. The absence of this linguistic principle also explains why children overgeneralize the causative rule to non-causative verbs as in examples such as "John giggled me" noted by Bowerman (1982). The interesting empirical result of this proposal is that it predicts the co-occurrence in real-time of several grammatical developments. Felix (1981) also proposes that there are a number of linguistic principles which are inactive at the initial state.

Apart from maturational factors which slow down the acquisition process, another difficult area is the lexicon, which contains a number of idiosyncratic features associated with particular lexical items, for example, argument structure, phonological form, subcategorization restrictions, and so on. Almost everyone would agree that much of what is in the lexicon must be learned largely on a item-by-item basis and that this takes time. More interestingly, however, there may be certain grammatical developments which are dependent on the learning of lexical properties. For example, it may be that the acquisition of sentential complementation is dependent on the child acquiring those verbs which take propositional arguments, such as think and believe. Suppose that existing between the lexicon and the syntax there are principles of canonical mapping of the sort proposed by Grimshaw (1981), for example, one which states roughly that "the canonical realization of a proposition is a sentence." The child learns the meaning of a particular verb and hence whether it takes a propositional argument. He then knows, by virtue of canonical mapping (and other principles of grammar), that S may be embedded inside VP. Prior to learning such verbs, he does not have that grammatical knowledge. Such an account might shed light on the experimental results of obtained by Goodluck (1981) and others that children correctly interpret subcategorized infinitival complements such as (7a), while they have great difficulty with non-subcategorized adverbial complements of the sort given in (7b).

(7) a. Pluto told Donald to jump up and down.
    b. Pluto hit Donald after jumping over the fence.

By hypothesis, the former are easier because the structure associated with such sentences follows from lexical properties of the higher verb, i.e. tell takes a propositional argument, though this is not the case with adverbial complements.

The central point is that although there are various innate mechanisms in place which allow the child to acquire complex sentences, the emergence of such sentences is delayed for reasons having to do with lexical/semantic development. Experimental studies of the child's interpretation of anaphors and pronouns, for example the work of Chien and Wexler (1987), suggests that lexical learning

may also be responsible for developmental delays in the setting of parameters associated with binding principles. In short the child must learn that himself is an anaphor and him is a pronoun before the relevant parameters can be set.

One final factor which undoubtedly influences the order of development of various properties of grammar is the formal complexity associated with the particular phenomena to be acquired. Thus, all else being equal, if property a is formally more complex than property b, b should be easier to acquire and hence emerge earlier than a. However, we need to be cautious at this point because as was pointed out in the early days of generative grammar, formal complexity is a theory internal notion and not an intuitive one. Within Government-Binding theory a distinction is made between the core grammar of a particular language and the periphery (Chomsky, 1981) The core grammar of a particular language is what results from fixing the set of parameters at one of the permitted configurations. Outside of core grammar is the set of peripheral or marked properties of the language. The periphery might include, for example, exceptions or relaxations of the settings of core grammar or idiosyncratic features of the language which are governed by particular lexical items, for example, the fact that in English verbs like believe allow "raising to object" so in the sentence "I believe John to be crazy." This construction is rather rare in languages of the world and within the theory of grammar it can only be accounted for by exceptional mechanisms.

In my own work, (Hyams, 1987) I have proposed that the core/periphery distinction explicates a number of aspects of real-time acquisition. For example, the acquisition of complex sentences, mentioned above. Children appear to first acquire the basic sentential phrase structure associated with complements, what we may think of as the core property of these constructions, and, as I suggested earlier, this may be done through a principle of canonical mapping. Only later do they work out those aspects of complementation which are peripheral, for example, whether the clausal complement to a particular verb be tensed or infinitival. Also, as predicted, raising and other constructions which the theory specifies as marked are relatively late grammatical developments. It is also proposed that the core/periphery distinction explains a number of properties associated with the acquisition of inflectional morphology. One empirical result seems to be that in those languages where inflection is a core property, such as Italian, children have an easier time acquiring the inflectional paradigms than they do in languages where inflection is a peripheral property, such as English and French. Moreover, the development of inflectional morphology can be shown to be directly related to the setting of the Null Subject Parameter (Hyams & Hyams, 1987), mentioned above. Without getting into the details of that analysis, the basic idea is that all children start out with a null subject grammar and it is by virtue of learning the core vs. peripheral status of inflection in their language that they either persist with a null subject

process or reset the parameter to disallow null subjects (Hyams & Jaeggli, in preparation). Thus, simplifying greatly, in Italian inflection is part of core grammar and it is able to license null subjects, while in English, it is peripheral and hence not able to fulfil this grammatical function. The analysis explains a range of acquisition phenomena, in particular, the shift to obligatory subject use in non-null subject languages, the emergence of tense and agreement inflection as it correlates with use of lexical subjects, the appearance of modals in English, and the Verb Second rule in languages like German. It is precisely this kind of "clustering effect" or co-occurrence of grammatical developments which provides some of the strongest support for a parameter model.

To sum up, then, we see that although there are a number of non-trivial factors which conspire to prolong the developmental process beyond the idealized instant, the empirical assumption embodied in the idealization, namely that grammatical development involves fixing a finite number of parameters based on positive data available in the environment, is consistent with the facts of actual development where these have been looked at in any detail. The picture of grammatical development that emerges on this approach is one involving a complex interplay of maturational, lexical (that is, learned), and grammatical factors, which is exactly what we would expect given the magnitude of the cognitive achievement involved.

The Role of Linguistic Theory

Language acquisition research within the framework I have been presenting necessarily proceeds in tandem with linguistic theory and this is where its strength lies, I believe. Any hypothesis concerning a particular linguistic development is accountable not only to the acquisition data and developmental principles, but also to the principles of grammar, which themselves have a broad empirical base. For example, the claim that the English speaking child has misset the Null Subject Parameter carries with it a range of empirical predictions which are derived from the linguistic analysis of the null subject phenomenon in adult languages.

Just as proposals concerning the structure of UG have implications for acquisition, so child language has implications for the linguistic analysis of adult languages and for the theory of grammar more generally. For example, if we note that children find some aspect of language A more difficult than a superficially similar property in language B we might argue that the phenomena in question should be analyzed differently in the two target languages. This is the substance of the analysis of inflection noted earlier in which I argue on the basis of developmental data that inflection may be part of the core grammar of one language but in the periphery of another. In a similar vein, Borer(1985) and Hyams (1986) argue for a reformulation of Binding theory based on evidence from acquisition, and Hyams

and Sigurjonsdottir (1988) propose a reanalysis of long distance reflexivization in Icelandic (mentioned earlier) based on experimental results obtained with Icelandic speaking children.

The road from acquisition data to linguistic theory is not always a smooth one, however. It is often difficult to argue for particular linguistic analyses based on child language data since there are a number of variables (linguistic and non-linguistic) which enter into the acquisition process and a priori any one of these could be responsible for the particular effects which we observe in the data. This is true for both naturalistic and experimental data. However, as we acquire a deeper understanding of those different variables which enter into language development, we are increasingly able to determine when certain acquisition data directly reflect grammatical knowledge and hence when they may well bear on linguistic theory. Thus, although the use of acquisition data poses certain problems, they are practical ones and do not involve questions of principle.

And what of the road from linguistic theory to acquisition data? It is often argued that linguistic theory is in too great a state of flux to be useful in the analysis of child language and that acquisition specialists should wait for the syntacticians to hand over the "right" theory of UG before applying it to the data of child language? The problem with such proposals is that the "right" theory of grammar must account for development (with the caveats noted earlier) and to a certain extent for use (since use of language is not totally divorced from knowledge of language) as well as those areas of more traditional concern such as grammatical variation. Thus, unless we explore the implications that particular linguistic theories hold for language acquisition and use and revise these theories accordingly we have not put the theory to the ultimate test.

There is one final consideration regarding the role of linguistic theory which I would like to mention. Chomsky (1965, 1986) points out that in the study of adult languages there is a creative tension which exists between the goal of adequately describing the range of variation exhibited by different languages and the goal of constraining the class of descriptive mechanisms in order to achieve a level of explanatory adequacy, that is, in order to restrict the set of grammars made available to the child so that he may converge on the right one given the available data. The same tension exists in the study of language development. We need to describe the stages of child language, that is, the variation which exists in the individual through time. At the same time, however, considerations of explanation require that the class of acquisition mechanisms be constrained in some principled fashion. In this sense the principles-and-parameters theory provides an explanatory theory of development in that it limits the number and kind of "developmental principles" to a set of independently motivated parameters plus lexical/peripheral knowledge.

Future Directions

As for future directions, I think that there are many fundamental questions which remain unanswered, some related to the structure of UG and others to the nature of the developmental process itself. With respect to the latter, we need to determine, for example, which aspects of UG are present at the initial state and which aspects are naturationally determined to emerge at later points of development? Another range of questions concerns the nature of the triggering data and how it is what is mapped onto particular parameter values. For example, do children operate according to a Subset Principle (Berwick, 1982; Wexler & Manzini, 1987) which states that they progress from smaller to larger languages? The answer to this question depends in part on whether parameters indeed generate languages which fall into subset relations. There are also a number of issues related to the core/periphery distinction, or the theory of markedness, however formulated. For example, are peripheral or marked properties of grammar acquired according to different (learning) principles than core grammar? How far can the core/periphery distinction go in explaining certain kinds of developmental variation that we find across children acquiring different languages?

Increasingly, people are attempting to explore adult second language learning within a parameter setting framework, the central question being to what extent is L2 acquisition like first language development. Notice that parameter theory provides a very precise way of formulating that question, namely, do adult L2 learners begin with a default parameter setting like the child does; the answer to this question is probably not. However, L2 may be like L1 acquisition in involving a resetting of parameters from the values assumed in the first language to those of the newly acquired language. As in the case of first language studies, there are strong empirical predictions which follow from this hypothesis, largely related to the clustering effect mentioned above. (For discussion of these issues, see Schwartz, 1986; Hilles, in preparation, and references cited in these works). A closely related area is that of simultaneous bilingual acquisition in children. Both bilingualism and L2 acquisition raise a very interesting question for parameter theory - namely, what does it means to have two coexisting grammars within this framework?

Thus far I have mentioned some open questions which exist in the developmental domain, but research in this area cannot proceed in a vacuum. The more we learn about UG itself, in particular about the kinds of parametric variation that human language exhibits, the more insight we gain into the nature of the developmental process.

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Panel

Theory and Explanation in Acquisition

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What I shall say is intended as a contribution to a debate, not a reflection of my innermost views. A debate has its own dynamic and its special value. It achieves a clarification of ideas by juxtaposition which in turn requires that a narrow stance be taken.

Everyone, in an easy programmatic way, can account for everything in general, but often very little in particular. If we have no deductive structure but only programmatic accounts, then we have made little progress. My goal is to present a set of theoretically derived theses on acquisition, but to present them so narrowly that it becomes absolutely clear that no approach, unlinked to linguistic theory, can possibly lead to an adequate account of the phenomena.

> *A) The concept of language changes as new data comes to light.* [90% of what is currently "definitional" in linguistic theory refers to concepts discovered in the last decade.]

It is a natural feature of a scientific field that its boundaries change as new insights arise. Chomsky (1986) has suggested that there may be no systematic definition or model of language at all, but only a systematic account of grammar. Numerous non-grammatical influences affect language--every aspect of cognition--while grammar remains a skeleton, hard and distinct, within a notion of language whose character is quite diverse. Consider two facts:

> 1. what did you file__ without reading__
> 2a. *who bought a house why
> 2b. who bought what

The existence of "parasitic gap" constructions (1), where one what seems to come from two different positions was unknown until Engdahl (1981) and Chomsky (1982) explored the phenomenon in depth. It has played a central role in the development of new theories.

Equally important (Huang (1982)) is the discovery of sentences which are ungrammatical, such as (2a) which contrasts with (2b). An argument wh-word, required by the verb, can be left in its original position and appear with another wh-word. However an adjunct wh-word, like *why*, *when*, *how*, cannot be left in the verb phrase and must appear sentence-initially. (In Asian languages there are important variations.) How does a child know that (2a) is impossible? We certainly cannot ask about its frequency of non-appearance. The frequency of appearance of the alternative, though, is very low. In the Adam corpus, for instance, I believe that there are no instances of (2b) over 3 and a half years.

The existence of these sentences changes the boundaries of grammar and changes what a theory of grammar must explain. One important role of linguistic theory is to uncover new data, forced into observability by powerful theories. The new data remain like rocks. They must be explained by any theory. The emphasis upon theoretical explanation has left this non-theoretical aspect of current work unappreciated: linguistic theory is a complex data-generating device. One can ignore or dislike linguistic theory; it seems impossible to justifiably ignoring the data which it brings into existence. If other approaches avoid this data, then we are not discussing the same object, we do not agree on what has to be

explained, on what has been acquired.

> *B) The crucial question for acquisition is not how the child learns which sentences are syntactically grammatical, but which interpretations are excluded.*

For instance, there is a difference between (3a) and (3b):

> 3a. whose shoes did he tie
> 3b. who tied his shoes

In (3b) we have a set of paired readings (bound variables) while in (3a) it is possible that John tied his own shoes, it is not possible for the answer to be John tied John's shoes, Bill tied Bill's shoes, etc. This is both a very refined and a very clear distinction. How does a child know that the first sentence cannot refer to bound variables? In fact Roeper et al. (1985) have shown that very young 3yr-olds do not give bound readings to (3a). At the same time their evidence showed that in long-distance environments, a bound variable reading was available where it should not be, in (4b):

> 4a. who thinks he has a hat (bound variable)
> 4b. who does he think has a hat (no bound variable)

Roeper et al. found that children allowed a bound reading for both (4a) and (4b) to the age of seven. [1] The question then arises: how do children eliminate one possible reading for (4b)? Note that the presence of non-bound readings for (4b), where someone ties another person's shoes, says nothing about whether the bound reading is possible. Another cardinal principle is involved:

> *C) The child cannot receive significant negative evidence.*

It is logically impossible for a child to receive negative evidence about an excluded optional reading. The issue has nothing to do with explicit aspects of syntax. No possible reading eliminates an optional reading. No frequency measure of possible readings is relevant to rare structures. Let us establish the point that structures are rare. In all of the Adam corpus we found only 16 examples of clear long-distance movement like "What he went to play with?" There were only 11 instances of the expression *whose* from Adam over three years; there were 35 for his mother. It is logically impossible that the child is computing non-frequency for interpretations. It is implausible that frequency is relevant to permissible and impermissible rare structures like those in (1).

In sum, it is the explanation of how a child acquires "invisible" information which is the heart of the acquisition problem. Any re-definition of the problem toward observable phenomena is a simplification and an essential distortion. Consider the acquisition of the past tense *-ed* form, which has been discussed by Rumelhart and McClelland (1987). The observation of where and when it occurs in child and adult language is a simplified gloss on a complex object. It is not surprising that there is a relation in frequency of gross appearance among adults and use among children. It has something to do with the frequency with which we choose to talk about certain topics. It may be frequency which, in a sense, brings a construction to the attention of a child. Frequency provides, however, no analysis. Much less does it explain the invisible features. Consider this example:

5a. the plant dropped
5b. the dropped plant

Both instances of (5) could refer to a situation in which an agent is present. We might say something like "the plant dropped when he let go of it". However the sentence (5a) does not refer to that agent, while the sentence (5b) can contain an invisible reference to an agent, an implicit agent. The sentence (5b) can be seen as a passive derivative. Passives (unlike (5a) also have implicit agents:

6. the plant was dropped

How does the child know, and when, that (5a) has no agent, while (5b) has an agent? This is the crucial question around the acquisition of the -ed suffix.

The problem is very real because in fact children do allow excluded interpretations and we do not know how they eliminate them. For instance we have assembled evidence that 3-4 year old children allow the elephant to be an agent in (7):

7. the elephant is pushable

It is something about the systematic nature of language which tells the child that the subject reading is excluded. In other words, the elimination of an interpretation can be accomplished only by application of a principle, not exposure to data.

The situation, from an explicit standpoint, is actually worse:

D. Input information is contradictory.

If there are exceptions to a rule, how does the child know that they are exceptions? In the realm of morphology, there are often several hundred exceptions. From the child's perspective, the input is contradictory. Consider the following examples:

8a. the purchasing of a car
8b. the buying of a car

9a. the purchase of a car
9b. *the buy of a car

All nominalizations with *-ing* are grammatical. The child must conclude that he can freely create novel *-ing* nominalizations. There are, however, hundreds of examples like (9a), often interpreted as results, but there is no guaranteed productivity, otherwise (9b) would have to be grammatical. [2] Nominalizations without an affix generally do not have a compositional reading. They undergo what one could call "instant drift". Thus *income* means only money. We cannot speak of the **the income of cold air*, although we might speak of *the outflow of cold air*. Thus we find that (9b) is not grammatical. How does a child know that *-ing* cases are extendable but not zero nominalizations? The decision must be made on

principle. The basic principle is simple: phonetic affixes are productive, and non-affixes are not (compositionally) productive. The principle cannot be gleaned from the explicit data which contains too much counter-evidence.[3] On the one hand, the child gets no evidence about invisible information, and on the other hand the child gets numerous exceptions which she must know not to use as a basis for generalization.

These problems lead to a heuristic for where acquisition theory is needed.

E) Subset violations show where principles apply.

A theory of language growth can follow the subset principle[4] in ways beyond grammar. A child has a structure, then hears a new one, and expands the grammar to include it. The term "hearing a new one" might be an idealization of a level of frequency. In addition, various pragmatic factors can be learned in the same way. The term "no" is used in a wider and wider range of pragmatic circumstances. When it is used in the context "no you don't say", then it practically means "yes indeed". A child can just add these new interpretations to the pragmatic domains where the structure is relevant.

When a subset violation occurs, though, then grammar change is probably involved. A violation occurs when a child allows two interpretations for a structure which has only one. If he allows these interpretations, then what will eliminate it? For instance, what enables a child to rule causatives (like "don't giggle me") out of his grammar after they are in.[5] One suggestion that I have pursued is that when children learn that the language has an object => subject rule, then it parametrically eliminates subject => object.[6] If a child hears "you giggled" and then says "don't giggle me", then he has, against the adult grammar, converted an intransitive into a transitive. What happens, then, is that a principle enters the grammar which causes a reanalysis of certain structures. The change is fundamentally indirect.

In the interpretive domain, we find that there are subset violations whenever a child allows too many interpretations. How can one be eliminated? What drives change? The only available answer is: a change in the grammar eliminates certain readings. Why does the grammar change over time? There are several possibilities:

F) the trigger is (a) ambiguous, or (b) parametrically contingent.

Or there is either cognitive or formal maturation. Each of these factors could lead to a wrong or partial grammar which had, as a consequence, the mis-analysis of certain data.

Let us quickly illustrate. The expletive *there* which putatively sets the pro-drop parameter is both an expletive and a locative as Hyams (1986) has argued. Unless a child hears a sentence like *there is a man there*, it is very difficult to know that the expletive is not a locative. The presence of expletive sentences in the environment is insufficient. The child must give the correct analysis to "primary data" before they are triggers. Because the analysis itself may depend upon parameters, i.e., vary from language to language, the grammar's growth may involve a real sequence.

Another example. Suppose a child does not know where a complementizer position occurs. It could occur on either the left or the right. Or suppose the complementizer position itself must mature. The child would then analyze questions as if they were topics. This could be a reason why children do not perform inversion, as in "what I can drive". It would be like "truck I can drive". Then the child cannot perform a question transformation which moves successively through a COMP position. Therefore, in the long-distance case, the child would be unable to move at all. The lexicon demands that a transitive

verb has an object. Therefore it must be filled. One way to fill it is with an invisible default pronoun. This appears typically in substandard dialects (*Who$_i$ did you think we saw his$_i$ brother and him$_i$*). For this reason, in the child's grammar, we would have (a) *who$_i$ did John put a hat on (him$_i$)*, instead of (b) *who did John put a hat on trace*. This would allow coreference between *who* and invisible *him* in just the way that *John$_i$ put a hat on him$_i$* can be coreferential. In sum, the interaction between misanalyses of the primary data and uncertainty over parametric settings will produce intermediate grammars which are incomplete and counter to the adult grammar. If we can explain how the misanalysis occurs and is eliminated, then we have a powerful, direct explanation of language growth. Such an explanation, but not a programmatic one, could serve as input to a theory of neurological growth.

This mode of explanation is crucial for the explanation of the acquisition of invisible information. It can, of course, be centrally involved in the acquisition of many visible phenomena. However wherever the phenomena are visible, numerous other factors will be attached, just as we attach numerous associations with any object, like a car. If we consider a word or a sentence to be like a car, then it is no wonder that we have a mass of confusing pragmatic and functional information that makes the attempt to figure out how a car works rather difficult. Cars involve beauty, value, functions, fears, physical comfort, and other things. A catalogue of the ways in which cars are used will not reveal how a carburettor works. Neither can a catalogue of the external features of language reveal the mechanism behind it.

One might construe this as a challenge to much of the cross-linguistic, taxonomic work in both adult and child language. It is not a challenge at all, but a caution. In every science, descriptive work provides the first step and much of the descriptive work must be abandoned or re-investigated as theoretical insights raise new questions. No one can visit an unusual place, gather data, and feel confident that he or she will not need a return trip.

The data which has been gathered in linguistics may fail, here and there, to provide an adequate technical analysis. Our work, though, has important general consequences. Details of cross-linguistic variation, sudden similarities between a far-away language and our own, serves to prove in minute fashion how subtle human language is, and hence how subtle human beings are. Thus linguistics intrinsically involves a respect for human beings. Acquisition research deepens our respect for children. We should never let scientific zeal diminish these moral values.

Footnotes

1. See de Villiers, Roeper, & Vainnikka (1988) for further discussion of these issues.

2. Note that "a good buy" is grammatical. It is precisely the full nominalization which is excluded. The notion of "blocking" will not work here since "a good purchase" and "a good buy" should block eachother. In addition, the concept of blocking is itself rather dubious, since it can always be escaped by refined gradations of meaning.

3. The principle itself requires the correct analysis of a word into stem+affix which may take time.

4. See Berwick (1985) for extensive discussion.

5. There is an extensive literature on this topic now. See in particular Lebeaux (1988) and references therein.

6. Originally proposed by Alec Marantz. See Roeper (1982) for discussion, although the domain is quite complex.

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Panel

THE CONNECTIONIST APPROACH TO LANGUAGE

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In this short piece I have space neither to offer a clear explication of the nature of connectionist theory nor to present a detailed description of any of the particular applications of connectionist models to language phenomena. Rather, I hope to give a qualitative description of some of the features of connectionist models which, it seems to me, fit nicely with features of linguistic information processing (For an introduction to connectionist modeling efforts see, Chapter 1 of our 1986 book *Parallel Distributed Processing, Exploration in the Microstructure of Cognition* volumes 1 and 2, Rumelhart, McClelland and the PDP Research Group, 1986; other chapters of the book describe in more detail examples of other connectionist systems.)

The process of building formal accounts of empirical phenomena always seems to provide a certain tension between one's pre-theoretical intuitions about the phenomena and the formalism or theoretical framework one has available to express those intuitions as a more or less formal theory. This is a process of compromise in which the richness of one's intuitions must eventually be pushed into the shape of whatever theoretical formalism or framework one has available. Once a formal theoretical account is developed, this account opens up a richness of its own and helps to further shape our intuitions. Most of our intuitions eventually seem to accommodate to our formal theories, but certain of them seem never to be satisfactorily accommodated and form the seeds for further theoretical development and perhaps the eventual development of alternative theories. It sometimes seems to happen that we become trapped by "the tyranny of notation," in which our original understandings of the phenomena we study become a slave to the formal system we have available to express our theories. I have been attracted to the connectionist modeling approach to language because I have found that it offers a formalism with a much closer match to my pre-theoretical understanding of language and communication than other existing frameworks for expressing these ideas. I will proceed with a discussion of some of my biases about the nature of language, language processing and language acquisition and then indicate how it is that connectionist models provide an appropriate language for stating theories consistent with these biases.

Language Processing as Constraint Satisfaction

One of the important features of connectionist systems is the ease with which they can be employed to find a state which is the "best fit" to a set of "soft" constraints. The notion of a soft constraint is one which is desirable, but not mandatory to satisfy. Each constraint has, in effect, a numerical value associated with its importance. Moreover, constraints interact, so that finding an interpretation which satisfies one constraint may well satisfy or violate others. The connectionist system is natural for finding solutions which, to the degree possible, satisfy as many of the most important constraints as possible. Hinton & Sejnowski (1983, 1986), Rumelhart, Smolensky, McClelland and Hinton (1986) and Smolensky (1986) all offer an explanation of how connectionist systems can be configured to solve such problems.

Much of language processing, in my view, can be characterized as a process of finding solutions to just such constraint satisfaction problems. The process takes a slightly different form in comprehension as opposed to production, so I will treat them separately.

Comprehension

The goal, in language comprehension, is to find an interpretation which is maximally consistent with the phonological, syntactic, semantic and pragmatic cues available in the current context. Each word constrains the meaning of the other in the sentence. I like to think here of the "detective" analogy in which the speaker provides a set of clues by his or her behavior in the current context and the listener, like a good detective, must take all of the clues together and construct an interpretation of the speakers intention as consistent as possible with the set of available clues. On this account, the meaning is not so much in the words, any more than the crime is in the clues, the meaning is simply the best interpretation of the available clues. Words themselves are not so much to be characterized by a meaning, as in a dictionary, but as set of "soft" constraints on the meaning of the sentences in which they occur. Thus metaphorical usages are not to be seen as different in kind from "literal" usages, it may simply be that the best interpretation of a metaphorical usage involves the violation of slightly more of the non conventional constraints in the situation. Cottrell (1985) and Cottrell and Small (1983) provide preliminary examples of how a set of constraints can be employed in the process of word disambiguation.

Production

The problem of production can also be viewed as a constraint satisfaction process. The speaker as a number of goals which must be simultaneously satisfied in the creation of an utterance. The idea is to create an utterance with has satisfies as many of the goals/constraints as possible. The speaker must find an utterance which satisfies a set of phonological, syntactic, semantic and pragmatic constraints. The connectionist formalism offers a mechanism for determining a sequence as consistent as possible with a set of constraints. Although there are no good examples of systems creating entire words, recent work by Michael Jordan (1986) provides an account of how a system can work at the phonological level (as evidenced by co-articulation effects) and in the motor system in general. I have begun to develop such an account at the level of discourse.

Language Processing is Interactive

Although some prefer to see language as a module somewhat separate from the rest of cognition and moreover to see language as created out of modules (phonological, syntactic and semantic), I tend to see the assumption of modularity as more several than principled - that is, I often seem to me that modularity is an assumption of convenience rather than principle of even empirical adequacy. (cf Fodor, 1983) I can see two several reasons for assuming modular structures. First, it is a useful way of breaking the phenomena to categories which we easier to deal with and secondly, it is difficult to most symbolic theories with complex representational formats to allow information represented in two rather different ways to intermingle in a single system. In spite of these conveniences, I believe that the empirical evidence on human linguistic information processing strongly favors the view of a set of mutually interacting systems in which information from each level contributes to the processing of each other level (c.f. Rumelhart,

1979, McClelland & Rumelhart, 1981, and Rumelhart & McClelland, 1982). Furthermore, connectionist systems are ideally suited for the development of interactive systems. The reason is essentially that there is a common "language" or "currency" of communication and exchange, namely information is conveyed entirely as levels of activations. Moreover, all representations are characterized uniformly as patterns of activations over sets of units. In this way, there is not incompatibility between, say, phonological, syntactic or semantic representations. Finally, since connectionist systems are inherently adaptive, it is possible for information from one source to impinge on another process and, through adaptation, for the receiver to attend to the information if it is useful or, to learn to ignore the information if the information it provides proves irrelevant.

The need for modularity causes us to focus on situations in which the phonological, syntactic, semantic and pragmatic aspects are relatively simply related. It causes us to ignore such phenomena as *sound symbolism, metaphor, partially productive idioms* etc. which call the independence of such subsystems into question. The connectionist perspective can, it seems to me, offer us the possibility of creating well specified, rigorous formal theories of such phenomena.

The Generativity of Language

One of the remarkable things about language systems is their generativity. We can produce and understand sentences that we have never heard before. Indeed, novelty would seem to be the norm in language processing. One of the strong points of traditional, rule based, accounts of language and linguistic information processing is the notion that knowledge of a relatively few recursive rules can yield infinite generativity. Yet, this is not the only account for this sort of productivity. It is, on the face of it, at least as plausible that new sentences are produced on the basis of analogies with existing old ones. To example if we understand that verbs like *send, carry, transfer* etc. take the dative as in *John carried the package to Mary* we can probably understand, by analogy, what sentences like *John walked the package to Mary* or *John ran the package to Mary* or even *John floated the package to Mary* might mean. As I see it, the biggest disadvantage of the analogy view is the relatively difficulty of formulating a clear theory of analogy or how it might work. The connectionist perspective offers a clear promise of how we might account for such analogical processes and hence for the generativity we see in language (c.f. Chalkley, 1979, Rumelhart & McClelland, 1986a and Rumelhart, 1988).

Language Acquisition

I take it that we can understand much about language processing by focusing on the process whereby language is acquired. One view has it that we should begin with the nature of adult language and then try to reason backwards from the adult language to a story about how that language might have been acquired. Although this can be a useful strategy it is possible that in the fluent adult aspects of linguistic information processing are hidden from view, but that in the child learning the language we can see these processes more clearly. Therefore, beginning with the child learning language and with a theory about how that language is learned might give us a clearer view :: over of the adult language. Now, at the heart of the connectionist theory is a theory of learning. Therefore, I suspect that some of the most important contributions of the connectionist approach might come from the development of models of the acquisition process. To date these models remain rudimentary. We have shown that the process of over-regularization and certain aspects of the so-called 'U-shaped' learning processes appears to

some aspects of language learning could result in a natural way from connectionist systems (c.f. Rumelhart and McClelland, 1986a) and Yves Chauvin has recently shown that there are analogues of many of the phenomena of early word acquisition which are a natural consequence of connectionist learning procedures. I should note here that although the focus in connectionist systems is often on the learning processes (because they are quite powerful and often produce surprising results) there is ample room for assumptions concerning innate, so-called "pre-wired" aspects of the linguistic information system. Indeed, one of the nice aspects of connectionist systems is that so-called innate knowledge blends in a natural way as a "starting state" on which learning may occur. Once learning begins to occur the difference between such innate knowledge and learning information is entirely historical: learned information simply modifies these structures laid down genetically in a way so as to make the question of what aspect of language is innate and which is learned to be mute since nearly all aspects would soon be a joint product of both the genetically determined start state and the experientially based changes to the starting state. See Rumelhart and McClelland, (1986b) for a somewhat fuller discussion of the issues of innate knowledge in connectionist systems.

Prototypes

Rather than clear boundaries among categories, it seems to me that most conceptual and linguistic categories are formed around central prototypes and variations on them. The boundaries themselves remain fuzzy. This point was made by Wittgenstein (1953), echoed by Rosch (1975) and more recently elaborated by Lakoff (1986) among others. It is a nice property of most connectionist models that this is precisely the nature of categorical information. Most symbolically based theories find it more convenient to hypothesize firm boundaries to fuzzy categories with little importance given to central cases. Biologically oriented models, on the other hand, would seem, intuitively, to begin with clear examples and proceed with more and more distant extensions with no clear determination of where the extension is no distant. Such a situation clearly seems to be the case with word senses and the general problem of polysemy. Here there seems to be a more or less central meaning and the various other senses nearly always are variations on some central theme or set of themes. In spite of the fact that dictionaries and most model lexicons contain a discrete number of word senses, it is unlikely that this is realistic. A much better account would seem to be a cluster of overlapping usages which can be extended or interpolated among almost at will. Prototypicality would seem to be present in such apparently discrete judgements as the judgements of grammaticality. Although there certainly are sentences which seem clearly grammatical and others that seem clearly ungrammatical there are many cases (often those which distinguish between theoretical accounts) it would seem which are unclear and subject to variation among speakers and even the same speaker from context to context. Even grammatical categories may not be as firm as we might like. For example, adjectives may vary on their degree of "noun-ness" and there are many other similar cases. Connectionist models promise to be able to provide good accounts for these phenomena. Kruschke and Anderson (1986), McClelland and Rumelhart (1985) and Chauvin (1986) have all shown how connectionist models possess prototype structure in category learning experiments. The same kinds of models should be extendible to the variety of cases of prototypes found in language and communication situations.

Summary

Although there is no complete theory of language or language acquisition here, there is a general approach to language which I find intuitively plausible and promising. This is a view which, in part or whole is, I think, appealing to many workers in the field. It is also a perspective which has historically been associated with fuzzy theories and imprecise specification. This has, in part, resulted from the fact the the best developed formalisms were not well suited for expression within the existing formal structures. I was, at the start, attracted to the connectionist approach as a potential tool for expressing and conceptualizing this alternative approach to linguistic information processing within a formal and rigorous theoretical framework. This task has a long way to go. Currently there are only a few micromodels of language relevant processes which have been developed within the connectionist framework. At present the results are promising, but it will be some time before such theories can become mature alternatives to the best developed theories of today.

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